AN

EXPOSITION

OF THE

PRINCIPLES OF PATHOLOGY,

AND OF THE

TREATMENT OF DISEASES.

BY

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TO

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THE FOLLOWING PAGES ARE INSCRIBED:

IN TESTIMONY

OF RESPECT FOR HIS TALENTS,

OF ESTEEM FOR HIS SOCIAL VIRTUES,

AND OF GRATITUDE

FOR THE UNMERITED OBLIGATIONS CONFERRED ON

HIS SINCERE FRIEND,

AND FAITHFUL SERVANT,

THE AUTHOR.

Bath,
April 12th, 1823.

PREFACE.

It cannot be said, at this time, that any one system of medicine prevails exclusively; or scarcely, that of the various ones extant, one is more extensively adopted than another. Indeed, so unsettled is the state of pathology, that those who read, are sceptics in all its doctrines; and those who do not read, are left to the guidance of a sort of intuition, which is not always productive of happy results; but very frequently suggests, through the course of a long life, only a reiteration of the same errors.

If the state of medicine is thus truly described, it appeared desirable to make a compendious review of these questionable doctrines: not in the vain expectation of defining their merits, or the true degrees of confidence to which they are respectively entitled, but in the more modest hope of exposing a few errors, and of establishing a

few truths, and of contributing, in a slight degree, towards the formation of a system of pathology which, resting upon the least exceptionable testimonies, may be generally embraced.

In a design so extensive as a review or exposition of the state of pathology, it must happen that the facts and opinions which belong to particular subjects are cursorily noticed, and sometimes altogether overlooked. This has occurred, not because such facts or opinions were lightly appreciated, but because the consideration of them did not belong to the plan of this volume; which professes to treat of principles, rather than of particulars; or, of the latter only, so far as they are necessary to illustrate or confirm the former.

As principles are abstract rules, inferred from the agreement of classes of phenomena, so the consideration of them, which proceeds less by a citation of individual facts, than by a reference also to classes of phenomena, in order to show their agreement or inconsist-

ency with the principles deduced from them, must necessarily have the character of speculative disquisition. If the subject were merely narrative, it would comprise only a detail of facts: but it is obvious that, as principles are propositions made by the understanding, so an examination of them belongs to the same province; and in a discussion of principles, experiences are to be compared, analogies examined, inferences to be made: and this process, which is familiarly expressed as one of reasoning, must of necessity be productive of speculative results. If exception is made against the examination of principles by reasoning, those who take this exception must be prepared to reject principles themselves of every sort: for these are formed by the same operations of the mind as those which are engaged in questioning, appreciating, or in refuting them.

In pursuing, however, as an avowed object, the examination of medical doctrines, the design of illustrating these, by as many facts as could be derived from my own experience,

has never been lost sight of; and it is probable that these references to experience, or these historical details, may sometimes compensate for the more laborious exercises of the understanding, which will be occasionally required by other parts of the subject.

From a few coincidences of opinion which I have discovered since this work was finished, I fear it will sometimes happen that conclusions are proposed with an air of novelty, which have been entertained and before publicly expressed by others. Wherever this discovery had been made, I have taken pains to disclaim the credit, which has been really due on the score of originality, though not of novelty: and, in order to avert the charge of intentional plagiarism, I would be understood to put in no claims for the merit of novelty; and content myself with leaving those, whether of novelty or of originality, to the ingenuous constructions of the reader.

I have merely to plead, in apology for the defects of reading, which I fear will be suf-

ficiently conspicuous, that the work was begun and finished during a journey, or in the course of a tour which occupied the principal months of the summer; at a time when I had much leisure, and but few books; when my library consisted of little more than an odd volume or two of Shakespeare and of Don Quixote. The few references which are made have been since added. It is presumed, that the obvious disadvantages which an author must suffer in composing a work under such circumstances, may be in some degree compensated by the necessity they impose of drawing his materials chiefly from his own stores.

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EXPOSITION,

&c.

CHAPTER I.

HUMORAL PATHOLOGY.

It is scarcely worth while to inquire what doctrines were held in medicine, or what practices prevailed, before the introduction of the mechanical philosophy. This happened shortly after the discovery of the circulation; when mathematical science was cultivated with enthusiasm, and when, from the great results which had already arisen from it, men thought its applicability little less than universal.

The hydraulic or mechanical pathology may be said to have been perfected by Boerhaave. Many fanciful and vague doctrines in medicine preceded this period; but our business is not with such as are exploded totally—whether those of the four humours of Hippocrates, the doctrines of the Magi and Astrologers, the heat and moisture of Galen, or the four winds and the planetary system of Paracelsus—but with the opinions which have descended to our own times, and which are still mingled with medical reasonings, and still direct

in some degree medical practice. It is our present business to examine this mechanical doctrine, in order to expose its errors, as well as to ascertain whether, amidst a general condemnation, it may not nevertheless be true in some particulars.

The humoral pathology is at this time seldom quoted, and never in the latitude which it once obtained. That part of the doctrine is pretty generally rejected which attempted the explanation of animal phenomena by the conflicts or harmonies of spherical or angular particles, by their adaptations to tubes of different calibre, by their remoræ, their mobilities, their errors of place, by their salt or oily qualities, by fluid particles which made acrimonious humours, and mucilaginous ones which served to obtund them. These phantasies are now pretty generally rejected: and even the addition of an acid and an alkaline nature to the fluids in question, the fermentations resulting from the conjunction of opposing influences, their attractions and repulsions, their conditions both peccant and laudable, together with all the hints derived from planes, levers, and screws, have not been sufficient to save this ingenious pathological structure from falling into that state of dilapidation and ruin which, sooner or later, is the fate of all things human. Yet, although this doctrine is not held in much reverence by pathologists, it is still the popular one: it appears more level to general apprehension than any other; and it is by no means uncommon for those of the profession to avail themselves of this popular apprehension, and to account for many circumstances, which

they are called upon to explain, upon false or absurd principles, rather than risk the suspicion of not being able to explain them at all. Thus they oftentimes gain credit for superior knowledge: and those who can condescend to accept reputation upon such terms are seldom deficient in the information, that an appeal to the fallacies and prejudices of human nature, is generally more successful than an address to its better understanding.

Although the humoral pathology has now in great measure given place to other doctrines, yet the evidences in favour of it are quite as conclusive as those for some of the opinions by which it has been superseded. It was seen that certain animal fluids, products of disease, were capable of producing in other subjects a disease similar to that from which they originated. It was seen, as in small pox, that the mode which nature employed to get rid of the disease, was to throw off the matter, in which it was supposed to consist, by the skin. The same was remarked of other cutaneous diseases, upon the occurrence of which internal ones were mitigated, or else ceased. The critical evacuations, as they were called, by sweat, stool, urine, or by abscess, all tended to inculcate the notion, that the causes of disease were certain humours, which produced the phenomena of the disease as long as they continued to reside in the system, and the evacuation of which was the mode of cure employed by nature, and therefore the one which was to be imitated by art.

The conclusion from facts of this description is

very obviously suggested: it could scarcely be expected that the efficient causes of such diseases should be looked for beyond that gross fluid which was seen to produce them. Men were the less disposed to do this at a time when the philosophy of materialism, although not confessed in the schools, obtained a very general belief. The notion that a morbid or peccant humour, which possessed a certain configuration of particles, working in a system confessedly full of minute tubes, and of incalculable complexity of mechanism, was the cause of phenomena with which it was so intimately connected, was a natural inference, the credit of which was to be questioned only when analysis had attained a more advanced stage.

Although, however, this humoral pathology is a doctrine which arises very naturally out of the contemplation of the facts just mentioned, yet it appears to be both inadequate and erroneous. The phenomena either of disease or of health can never be resolved into the movements of mere mechanism.

We will suppose there to exist in the system a peccant humour, or morbific matter; in what mechanical way can it produce the phenomena of pneumonia, or of synochus? We will say, that this fluid produces disease by its action, for the sake of simplicity, upon the heart: what is the *modus operandi* by which the configuration of the particles of a fluid can, by a mechanical agency, make the heart beat 130 instead of 70 strokes in a minute? In order to produce such a change

in the action of the heart, the fluid must have some relation with its *moving powers:* if these powers are influenced mechanically by such a fluid, they must themselves be of a mechanical kind; but, as the known mechanical powers of motion are such as arise either from springs or weights, and as the existence of such powers cannot be recognised in the construction of the organ in question, it is necessary to look for other causes of an effect, with which those which have been assigned have no sensible or analogical connexion. If these causes, then, of the action of the heart are not of a mechanical kind, it is necessary, as their existence is inferred from their effect, to distinguish them by some other denomination.

By whatever term the causes of the action of the heart may be expressed, if they are said to be not mechanical, then our peccant humour, or morbific matter, if indeed it is at all interested in the effect, produces the change from 70 to 130 beats in a minute, not by a mechanical mode, but by a relation with actuating properties which are confessedly not mechanical. Hence, conceding the existence of such a morbific matter, if its mechanical relation can be only with a mechanism, which does not exist in the present case, its inadequacy to explain the phenomena imputed to it in this instance is sufficiently obvious; and its fallacy is demonstrated by the proofs of a dependence of these phenomena upon powers which, having no analogy with those of mechanism, must be otherwise designated.

The doctrine of the humoral pathologists re-

gards matter as originating disease: the analysis which has taught us to look, beyond mechanism for the explanation of animal phenomena, to another set of powers, has taught us also to regard these same morbific matters of the humoralists, not as the causes, but as the effects, or products, of disease. In all instances in which the order of occurrence is perceptible, in the generation of the morbid poisons, the disease precedes the formation of the matter: thus the immense pustular eruption in small pox is the consequence of the fever incident to this disease; and thus irritation, in all other instances, precedes, and does not follow, the purulent or lymphatic secretions.

It appears, then, that the properties of morbific matter, which, according to the humoralists, depend upon the configuration of its particles, can have no direct relation with the principal powers concerned in the phenomena of disease: yet it is sufficiently clear that the qualities of such matters, of those which in modern phraseology are called morbid poisons, are capable of producing diseases of the most formidable character, and even death. As the qualities in question are not reducible by any analogies to those either of mechanism or of chemistry, they must be considered as belonging to some other class; and from their being related only with the properties of living animals, so as to produce those effects by which they are characterised, the denomination of vital properties may be adopted with apparent propriety.

The existence of those which we have called vital properties is found to prevail most exten-

sively throughout nature. They are not confined to living forms; but are discovered so generally in the inanimate substances, that there are perhaps very few which do not in some degree possess them. They are in alliance with the common properties, and with the chemical ones of matter; and their existence in the inanimate substances is sometimes shewn by the circumstance that the vital properties of living animals are renovated from them, in the way of nutrition. This proof of the existence of a class of properties in some inanimate substances will be considered sufficient: but it will be remarked, that those substances which are proper for the nutrition of living bodies have themselves been possessed of life, either in the animal or vegetable form. The derivation of vital properties from inanimate sources is not, however, the less certain, from the circumstance that living animals are supported by structures which once possessed life in some shape: the only difference is, that those properties are derived by men from the earth mediately, rather than directly.

But, if those substances which support the life of animals by nutrition are on this account admitted to contain vital properties, it will be urged, their existence in other inanimate substances (as, for example, in the morbid poisons in question) is not to be evinced in the same way.

We find that the inoculated saliva of a dog will produce hydrophobia; that the bite of a serpent will produce inflammation of a limb, gangrene, and universal death; that inoculation with certain

morbid poisons, obtained from one subject, will produce a corresponding disease in another; that if some vegetables maintain life, it is the property of others to destroy it; that of the chemical properties also, some promote health, or contribute to life, others occasion disease, and others are speedy in producing death. Are, it will be asked, all properties to be considered vital, whether their operation is to maintain life, to affect life, or to destroy it?

In order to discuss this question, we must consider what happens when life is influenced by either of the properties alluded to. It is consonant with our experience in causation, that, if one thing modifies another, it is by combining with it: in which case an effect is produced, which is the united existence of the causes. To speak less minutely on this point, it will be conceded, agreeably with the laws of ubiquity, that one thing cannot influence or change another, as long as it is separated from it: hence, any one of those properties just mentioned must unite with life, and change its form. So far, therefore, as a relation of affinity with life might sanction the appellation of vital properties, all those which manifest this affinity may be said to belong to this class. But chemical substances exhibit the same relation of affinity: life is as readily affected, or its state as completely changed, by arsenic or corrosive sublimate, by withholding oxygen, or respiring carbon, as by the introduction of the animal poisons.

As certain properties of chemistry are necessary

for the support of life, and as other properties of the same kind are capable of affecting or destroying it, it appears that these properties cannot well be appropriated either to the department of chemistry or of life. In this dilemma, it appears proper to limit the class of vital properties to those which have no other denomination, and which, not being discoverable by the tests of chemistry, and producing on its substances none of those mutations which are accomplished by recognised agents of this class, appear to require a separate title. At the same time, it must be observed that our analyses are at present very imperfect; and that therefore the phenomena which we ascribe to a single agent, are often not produced by this agent, or by any thing like it, but by latent properties which belong to this agent, and either help to constitute it, or else are only associated with it. Thus, if the classes of chemical and vital properties in a substance are totally distinct, that property, for example, in atmospherical air, which maintains life (recurring to the theory of oxygenation, which I still believe to be true in part,) may be a vital one, in alliance with oxygen; and by no means identical with the chemical one, which performs the agencies of the laboratory. Thus also, properties of life may be in alliance with electricity, and not identical with this substance. Such a distinction, although it may be reasonably suspected, cannot be demonstrated; because, so far as our analyses have gone, electricity appears to be a simple substance, although our inferences on causation would teach us that it is, like all

other things, composed of an infinity of forms of existence. No one doubts that vital properties are contained in the earth, and are not identical with it: yet we attain our knowledge of this distinction only because the vegetable forms of existence are capable of separating, from the gross material which we call the earth, those vital properties, which, by this act of analysis, are proved to be only allied with it. As our analyses proceed, we shall find many instances in which causes have been erroneously assigned. In the mean time, imputing phenomena only to such causes as are recognised, we must assign a relation to subsist between those of chemistry and of life, by which their forms may be modified, or their characteristics destroyed.

It appears then, in the case of the peccant humours, that their phenomena are not produced by any mechanical agency. It is more agreeable with the results of analytical inquiry to conclude, that the animal poisons contain latent properties of a vital kind, which are related with those of the same kind in living bodies; that the phenomena of disease or death, which ensue from the operation of the animal poisons on living bodies, are according to the nature of the properties which are engaged in this relation. It seems proper, in all questions which respect the nature of the properties to which animal processes are to be assigned, to consider those agents as belonging to the department of life, whose operation cannot be explained by any perceptible analogy to the agency of chemistry or of mechanism.

It appears then, that certain animal fluids are endowed with the qualities of poisons, by latent vital properties which are allied with the fluid material. This affinity between these subtile properties and the grosser fluid, prevents the dissipation of the former, and preserves also the characteristics of the latter; it serves at once to limit and to extend the relations of the poisonous qualities: by it these qualities have a fixed place; and by the medium with which they inhere, they are capable of mingling with the fluids of animal bodies, and of being, in this way, conveyed into the circulation. It appears probable, that by the existence or absence of this affinity between the active properties of the animal poisons, and a fluid medium, the distinction may be furnished between the contagious and the infectious diseases. According to the entire absence, or the degree of this affinity, we should find some diseases which are wholly infectious, others which are both infectious and contagious, and others which are only contagious.

It is sufficiently obvious, that those actions which characterise disease, cannot be produced, directly at all events, by any mechanical operation of particles composing fluids. Although the humoral pathology may be justly rejected, so far as it assigns a mechanical agency, where the organs concerned are not liable to be acted upon in this way; yet a conformity of the fluids with the calibre of the vessels through which they are destined to circulate, is a belief which is generally admitted, if not defined, by pathologists. Thus,

it is common to imagine a fluid too thick for one order of vessels; to speak of blood being infarcted into vessels too minute to be freely permeable by such a fluid—it makes a part of the notion of congestion; and this opinion, so far as it is admitted, implies little less than the error loci of the humoralists.

The opinion insinuates, that a fluid of gross particles may be forced into tubes which are capable of conveying only a fluid of a more subtile kind; that as it is the structure of these tubes to diminish in area towards their extremities, certain fluids may be impelled by the force of the circulation into such tubes, and become fixed in them, giving rise to a congestion in those parts of the vessels so circumstanced, which are nearest the trunks from whence they originate, and also forcing an undue proportion of fluids into unobstructed vessels.

Whether such a relative state as that just described between the fluids and the capillaries does or does not ever occur, is perhaps more than we can peremptorily decide. That a conformity of the fluids with the area of the vessels through which they are to circulate is necessary, is sufficiently proved by the results of injections; in which instances we observe that fine injection will fill vessels in which coarse injection will not run; and that by means of quicksilver, which has a still more minute divisibility, an order of vessels may be injected which can only in this way be displayed. Yet, that the same thing occurs in the living body, is not to be positively deduced from this analogical testimony.

We observe in the living body, that the fluids which permeate the capillary terminations of the arteries, are their secretions; and that of these, some are gross, and others thin. Now, if the gross cannot permeate the tubes which are destined for the thinner fluids, the thinner fluids may always permeate the tubes which are adapted for the more gross: this would be the case, if fluids were admitted into tubes merely according to hydraulic laws. Thus, vessels which secrete mucus, should admit serum still more freely; and the seminal secreting vessels should transmit readily either serum, mucus, or blood. It appears from this fact that, in the living body, the laws of hydraulics are superseded by the functions of life.

In further opposition to the opinion of an error loci, or that fluids get into vessels through which they cannot pass, it may be observed that the fact was never witnessed, which it might have been, if true, in the course of microscopic observations, for other purposes perhaps, of which the structures of living bodies have so frequently been made the subjects. It may be remarked also, that the vessels of the conjunctiva, when in inflammation these vessels are injected with blood in the greatest possible degree, bleed freely when cut; and that in syncope, they empty themselves of their contents, which they would not do if the blood they contained were in the circumstances supposed by the doctrine of error loci.

It is scarcely possible to admit that an error loci, or an obstructed state of the vessels, should occur in any other way than by means of the secretions

which certain vessels are disposed, by a natural function, to separate from the blood. It is not probable that a thick fluid should enter vessels which are adapted to convey only thin ones, because no separation of fluid could take place from the blood, except by a secretion, which supersedes hydraulic laws; and by the exertion of this power of separation, each set of vessels would obtain only their own secretion.

It appears then, that vessels can become obstructed only by the admission of blood itself into the order of the capillaries, or that the vessels of secretory structures may become obstructed by the fluids which it is their function to produce from the blood. We have seen in the example quoted of inflammation of the conjunctiva, that it is contrary to perhaps almost the only fact which can be cited, to suppose that blood, which is not extravasated, is ever in a fixed state, or one of rest: whether it is equally certain that secerning vessels are never obstructed by their own secretions, remains to be examined.

We have experience that the ducts of a secerning structure, may become obstructed by a product of the fluid they separate. The most unequivocal example of this, is the obstruction of the biliary ducts by concreted bile. But in this instance it arises out of the structure of the organs, that bile should be extravasated, or poured into the gall-bladder; a natural receptacle, where it remains at rest. It cannot be legitimately inferred from this example that the secerning structure of the liver is liable to a similar obstruction: the

case is parallel to that of the blood, which we find always fluid and circulating, while it exists in the system of the circulation, and is impelled by a *vis a tergo;* and coagulated, when in a state of rest; unless it is rendered fluid again by the processes of chemical change, or perhaps decomposition. The obstruction, therefore, of secerning tubes by the fluids they separate, in this most received example, is not only deficient in proof, but appears to be contrary to analogy.

The seminal secretion is said to become inspissated, and to obstruct the vessels of the glands which produce it. Of this, I presume, there are no examples: the supposition amounts, probably, to no more than a vulgar conjecture.

Ranula has been attributed to an obstruction of a salivary duct: but, as I have known this disease in an extraordinary degree to occur in consequence of exposure to cold, which was followed by fever, general tumefaction of the face, and an inflammation of submental glands which ended in suppuration, it appears more probable that ranula is in general produced by an inflammation and consequent thickening of a salivary duct, in the same way as a cold thickens the membrane of the nasal or Eustachian canal, producing epiphora, or deafness, than that the excretory duct in question becomes obstructed by any state of the fluid which it is destined to convey. It is stated that hydroglossus is sometimes occasioned by calculus in a salivary duct: it has been mentioned that the calculus has been cut down upon, pushed back with a probe, &c. I do not

know in what proportion of instances the disease might have occurred from this cause. I have seen many cases of ranula, but never had reason to impute it to an obstruction of this kind: at the same time, as calcareous deposits are by no means uncommon in scrophulous tumours, it is perfectly possible that these might occur in a salivary gland, and, passing from thence into its duct, might produce the disease in question. The duct of the parotid gland, it is said, is not liable to obstruction from this cause.

In the instances, also, of urinary calculi, the concretion does not take place in the uriniferous structure, but in the pelvis of the kidnies, in the bladder, or in a sulcus of the prostate gland, where a lodgment might take place, and where the fluid, separated by a process of secretion, is no longer impelled by the force of the circulation.

In scrophula, also, it is common to speak of the glands of the mesentery, or of the neck, being obstructed. They inflame, and swell, and suppurate: if they become obstructed, it is more agreeable with analogy that their obstruction arises from a thickening of their structure, and an obliteration of their tubes by inflammation, than that it is occasioned by fluids too gross to permeate them.

Uterine obstructions are also spoken of: these will probably be placed, with more propriety, to the account of vicarious determination.

The example of obstruction of a secerning structure, which has obtained the most extensive credit, is that of the liver. On this it has been

remarked, that the proofs are equivocal or defective, and that the supposition is contrary to analogy. But if the opinion should be admitted, the circumstance would then only prove that the secerning system of the liver produced a diseased secretion; for the vessels would not be obstructed by a healthy one, and if the secretion is one of disease, to what must this be imputed but to a diseased action or state of the vessels by which the secretion is produced? Allowing, then (which is not intended), that the secerning system of the liver might become obstructed by the fluid which it separates, and which is still contained in tubes which subject it to the impulse of the circulation, —allowing this, the secretion has then only the force of a re-agent in disease, the primary cause of which is a disordered state of those powers of life by which the separation of bile from blood is accomplished.

I am not aware that the analyses of biliary calculi have served in any great degree to explain the circumstance of their formation. It is a first-sight sort of opinion, that a secretion of bile which is preternaturally inspissated might explain the formation of gall stones. We might, perhaps, as well assign the same reason for the formation of urinary calculi; except that, in the latter case, it is obvious to the senses that thick urine is secreted, and calculus is not formed; and the contrary might sometimes happen. We cannot explain the formation of urinary calculi without calling in the aid of a disposition in urine to this separation: a peculiar disposition also in bile

must be allowed, in order to explain imperfectly the formation of gall stones. The explanation is not complete in either case, unless we are able to say by what previous change in the secreting organs the material of these calculi is formed; whether as a new product, or a natural one, in excess.

As all processes in the animal economy must be traced to a primary change in those properties of life without which the materials would follow the fate of dead animal substances, it is to be presumed that it is among these properties of life, and not in any state of the secretion, which can have only the force of a re-agent, that the cause of the phenomena in question is to be found. Whether supposing the secretion of bile or of urine to be healthy, the absorbent function of the structure in which calculi are found may be supposed to have any agency, as by a partial separation from bile, or urine, of some constituents, leaving a concreted residuum, is a conjecture, for the decision of which we want facts.

It appears, from this view of a pathology which it is professed to regard as exploded, although the explanations it affords are still occasionally resorted to, that the fluids throughout the vascular system, whether in the blood-vessels or in the capillaries, are never at rest: that obstructions and remoræ cannot take place from the injection of fluids into an order of vessels too minute to be permeable by such fluids; and that, therefore, the phenomena of disease which have been imputed to this cause cannot be so produced. If this universal efficacy

on the part of the animal fluids in disease is denied, what sort of adaptation of the hydraulic kind may be considered as necessary to health, or what deviations from this adaptation may be productive of disease? Having premised that the conditions of the fluids are governed in some stage by a primary agency of the properties of life, and that their varieties, whether of quality or distribution, are also secondary, by which they acquire only the import of re-agents, we may proceed to the consideration of these questions.

It has been, and still is, a prevalent opinion, that the blood may be too thick, or too thin. That the blood is either preternaturally thick, or preternaturally thin, is no speculative point: it is capable of a sensible testimony. Our experience proves, beyond a doubt, a considerable difference in this respect in different individuals, or in the same individuals at different times. That which has occurred to me in this respect, must also have been generally remarked by others. It is no uncommon circumstance to find blood taken from a vein of a very dark colour, and flowing almost like thin treacle, or pitch. Such blood may be remarked, upon separating, to contain but a very small portion of serum, and a crassamentum in general of weak cohesion. On the other hand, it is common to find venous blood, which consists of a small proportion of crassamentum, which, if the blood is uninflamed, is also of weak cohesion, and a very large proportion of serum. In those who have been frequently bled in the course of a disease, this latter state of the blood is often very remark-

able in its degree, excepting, that as repeated bleedings are commonly pursued in order to subdue an inflammatory disease, the crassamentum is tough, cupped, and covered with fibrine. Blood taken from a vein after a profuse hemorrhage, followed, perhaps, by determination to the head, and delirium, is also found to consist of a very small portion of crassamentum, which floats in a large quantity of serum. Thus much for the facts.

The relation of blood with the heart is certainly one of a mechanical kind: it is the opposition of a weight to an active power. Agreeably with this relation, it requires a greater exertion of the powers of the heart to give the same velocity to a thick than to a thin fluid, supposing the bulk to be equal. Hence, unless the heart acts with proportional vigour, there will be a slow and laboured circulation where the blood contains an excess of crassamentum. I am not aware that this circumstance singly would produce any other disease. So far as the mere rate of circulation is concerned, a pulse which ranges from fifty to sixty-six, is as compatible with health as one which ranges from seventy-two to ninety: and we know from experience, that the secretions which contribute to functions, as the gastric, intestinal, biliary secretions, &c. are as copiously furnished by blood in this state as in any other: at least, we have no reason to presume the contrary. But with this state of the blood there is commonly some disposition to disease, which cannot be attributed to the rate of circulation, or arise out of the relation we have defined. The person in

whom I remember this state of the blood to have existed in the greatest degree, was dyspeptic, subject to vertigo, and temporary confusion, if not alienation, of mind.

The symptoms just mentioned could not arise from any relation between this state of the blood and the action of the heart: it is not, however, impossible that the brain, the function of which is so highly susceptible of the influence of pressure, may suffer by this mode, from the preternatural weight of the blood in its vessels. It does not appear that any other organs are likely to have their functions impaired in the same way: they suffer from congestion, and from inflammation; but the former state supposes the vessels to be distended by a preternatural quantity of blood, and the latter is a state of disease which is made up by many circumstances, which cannot arise out of hydraulic relations of any sort.

In those instances in which I have remarked the blood to contain a preternatural quantity of crassamentum, the persons have frequently, perhaps generally, been bled for the relief of the head, which seems to agree with the conjecture that the weight of the blood in this state may disorder the functions of the brain. And it is but reasonable also to conclude, that the effects of particular determinations of blood, or of congestions, whether in the brain or in other organs, would be increased in their degree by the cooperation of blood which possessed more than ordinary gravity; adding the operation of pressure to that of distention. But, without this particular

determination just supposed, we find that the other organs perform their functions easily; and that this is the case with the lungs, which, next to the brain, may be supposed, from the nature of their function, and from their compressibility, to feel the most readily any increased gravity of the blood which circulates through them.

The same view of a mechanical relation between the blood and the heart must obtain, when this fluid is preternaturally thin, as under the condition of it which we have just considered; that it is the opposition of a weight to an active power. The proportion of serum to crassamentum is sometimes more than may be thought natural, in persons who are in tolerable health. But we are chiefly to consider what effects may arise from this thin state of the blood, in those extreme cases in which, from previous losses of this fluid, the crassamentum bears a proportion to the serum, as perhaps one to five. We observe, in these cases, that the active and resisting powers are rarely balanced: the heart retains its power of action and operates upon a diminished weight; there is seldom a slow pulse in those who have lost much blood, either by the lancet, by an accidental wound of an artery, or by uterine hemorrhage. On the contrary, the pulse is apparently full, bounding, and ranging perhaps from 90 to 120; and, if under the latter rate, is easily raised up to it by the most trifling excitement. I know not in what way this accelerated action of the heart, and apparently vigorous circulation, after immense losses of blood, are to be accounted for, but by supposing that the

power of the heart, its irritability, or however else denominated, is not diminished by such loss of blood; or, if diminished, that it is not in a ratio to the reduction of weight against which it acts.

As we have remarked vertigo and alienation of mind to have been connected with a condition of the blood in which the crassamentum was preternaturally abundant, so it also happens that the head is most commonly affected in this reverse state of the blood, though the symptoms are not precisely the same. It is commonly, and in my own experience it has been I believe invariably, the case, that those who have sustained great losses of blood, suffer more or less from what is called determination to the head. The symptoms most commonly are intense pain, and throbbing in the forehead or back part of the head, with a pulse seldom under ninety. I have known these symptoms to proceed on, with a pulse from 120 to 140, to delirium, serous apoplexy, and death. As this has occurred in a case where the loss of blood was not connected with any previous disease; so the determination, if it may be so called, in this instance, could be fairly imputed to no other cause than the loss of blood.

The mode in which the symptoms are produced in the instances alluded to, will be a subject of future consideration. At present, it may be sufficient to suggest as alternatives, either that the blood-vessels of the head are in some way preternaturally affected by hemorrhage; or, which is connected with our present topic, that the pain and more moderate symptoms above described, and which are chronic in those who have suffered

great depletions and still continue to lose blood occasionally, are the consequences of blood being driven through the vessels of the brain with too great an impetus, irritating the structure by the rapidity of its motion, and acting on the parietes of the vessels in a way which is somewhat analogous to over distention. It will be recognised that this latter mode is one which might arise from the excessive tenuity of the blood, by which the resistance of this fluid to the powers of the heart is preternaturally diminished. The coldness of the feet, which generally accompanies these symptoms, indicates that, if the velocity of the circulation is increased by a disproportion between the active and resisting powers, the distribution of the blood is also, from other causes, irregular.

It is observed after profuse hemorrhages that dropsy sometimes occurs, and this circumstance has been attributed, recurring to the humoral pathology, to the thin and watery state of the blood. But as this state of the blood is invariable after great depletions, and as dropsy is, compared with the frequency of the cause assigned, a rare occurrence, it is to be presumed that this disease, although connected with such a state of the blood, is dependent upon something more than a mere hydraulical agency. The hydraulic explanation supposes, that water abounds in the blood, and that it runs freely through the secerning extremities of the arteries, and is poured into the cavities. If nothing more were required than this simple process to produce dropsical effusion, as the cause is an invariable one, and the mechanical relation always the same, this effect would in-

variably succeed to the condition of the blood upon which this theory supposes it to depend. But as this succession is far from being regular, but is rather, upon the whole, unfrequent, it is necessary that we should consider the occurrence of dropsy rather as governed by the laws which regulate secretion, and which supersede those of the hydraulical kind, than that it arises out of a relation which is merely mechanical. Hence, as dropsy is sometimes a consequence of profuse bleedings, and more frequently does not succeed to them, it is necessary to suppose that the secerning function is more or less impaired in the several examples. It will perhaps be conceded, that this impairment of function consists in a modification of the properties of life, which have their seat in the secerning system. Whether we can attain a more precise knowledge with respect to the nature of this modification of vital properties, it makes no part of our present examination to inquire. It is frequently remarked that the blood taken from a vein, in persons who have lost considerable quantities of this fluid, is of a florid colour, and emitted with a jet. It appears possible that this colour of the blood may be from a diminished quantity of colouring matter which the blood loses, in proportion as it is deprived of its crassamentum; as we find that a colouring matter which, when concentrated, is of a deep red, becomes more florid and paler in proportion as it is diluted.

The emission of blood from a vein with a jet, corresponding with the systole of the heart, may also arise from a state of excessive tenuity of the

blood, which subjects it to a stronger impulse from the heart; and, from the diminished resistance of the fluid, to a more easy propagation of this impulse throughout the circle of vessels, than obtains when the blood contains more crassamentum, and, possessing greater gravity, is consequently less susceptible, in remote points of the circulation, of the impulse given by the heart. When the action of the heart is exceedingly vigorous, I have remarked both these circumstances to occur, at a first bleeding, in very robust men: the blood from a vein has been of a bright red, and has been emitted with distinct jets. An action of the heart, which is one of more than ordinary vigour, is, with respect to blood which possesses its natural quantity of crassamentum, the same as a weaker, though still, perhaps, a disproportionate action of the heart to blood, the specific gravity of which is diminished by privation of its crassamentum. At the same time, the sole agency of the heart in the circulation is not to be concluded from this circumstance; for the impulse of blood, given by the heart, would scarcely be extended to the veins if this were the *only* power of the circulation.

That this explanation is the true one is indicated also by the fact, that the blood from a vein, which was at first dark, becomes of a lighter colour when perhaps sixteen or twenty ounces have been abstracted. I have also seen it emitted with a jet, only towards the end of a depletion, the effect of which is to diminish the resistance to the action of the heart, and to facilitate the

transmission of blood from the arterial into the venous system. The means, therefore, by which these circumstances are produced, although not the same, are, however, in either case equivalent in regard to their end.

The rapidity with which the abstraction of blood produces a change in the state of this fluid is a phenomenon frequently remarked, but not to be, perhaps, satisfactorily explained. The blood taken by a second bleeding is commonly found to contain a larger proportion of serum than that taken by the first; or even the blood taken in a second or third cup, at the same bleeding, may perhaps contain less crassamentum than that in the first. The proportion of crassamentum to serum diminishes afterwards in a ratio to the depletion, and the quantity of blood abstracted. According to the fact, it appears that the blood escapes first which contains the largest proportion of crassamentum; yet we cannot conceive, in the first place, that the entire quantity of blood existing in the system at the same time is not one homogeneous fluid; or, in the second, if there were two fluids, that one should regularly escape before the other. There appear to be only two modes by which this sudden change in the consistence of blood can occur: one is, that it is the immediate effect of bleeding to produce an absorption from the cavities into which serum had been poured, which is as suddenly conveyed into the sanguiferous system, and augments the proportion of serum in the blood; the other, and less probable mode is, that, from the effect pro-

duced by bleeding on the properties of life, they may exert a power of decomposition on the blood, by which a larger quantity of serum may be separated from a given quantity of blood.

The former explanation, if either is adopted, will certainly be preferred: at the same time, the change produced in blood, during its abstraction, is not merely confined to the proportions of serum and crassamentum. It has fallen under my own observation repeatedly, and, no doubt, under that of most others, that the crassamentum of blood taken in a first cup has been covered with a layer of fibrine, of perhaps the eighth of an inch in thickness, while that in the second cup has not exhibited the slightest marks of inflammation, although both cups were filled by a stream of no perceptible difference. I am not aware that this change can be accounted for, without supposing that the immediate effect of the bleeding is to subvert that state of the properties of life which is the efficient cause of inflammation, and to which, it may be presumed, the exhibition of fibrine on the surface of the crassamentum is to be imputed.

The circumstance of the increased proportion of serum to the crassamentum of blood, as a consequence of repeated bleeding, appears to be more satisfactorily explained by supposing an immediate absorption of fluids, which are conveyed into the blood-vessels, than by any other process. The principal source of the supply of serum, in such cases, must be from the cellular membrane. If this supply were thought to be inadequate, it

may be conjecturally suggested that a regurgitation of fluids from the secerning systems might also take place, and that the evacuation of them by cutaneous perspiration, by exhalation, and secretion, might be, at the same time, suspended or diminished. It is, in the case we are considering, necessary to find some source by which the preternatural proportion of serum might be supplied.

In those chronic cases in which large bleedings have been employed at first, and smaller ones afterwards, in a long course of treatment, we have no difficulty in assigning the large proportion of serum in the blood, at least in part, to the means of nutrition, which belong to the plan of treatment. These are, in general, chiefly fluid, perhaps wholly vegetable; a diet, from which, it may be presumed, in agreement with a few facts, crassamentum can be but slowly accumulated. But in that sudden disproportion, which occurs even during bleeding, we can derive no explanation from any circumstances connected with digestion. It is, on this account, positively necessary to infer some change in the system, as an immediate effect of bleeding; by which either fluids which have been separated from the blood are conveyed back again into the circulation, or by which an effect is produced on the relative constituents of blood by an influence of life, which is of a more vague and mysterious character.

Conformably with an observation which has been rather limited, the crassamentum has appeared to abound, in the most preternatural ex-

cess, in persons whose diet has consisted almost entirely of animal food, which has been taken habitually in very large quantities.

We have considered the states of preternatural thickness and tenuity of the blood in the mechanical relations of this fluid; and the importance of these varieties, although not to be wholly overlooked, does not appear to be very considerable, either with respect to the preservation of health or to the production of disease. The history of these states is, that they are the results of previous disease, or of preternatural influence, as that of treatment: and their mechanical importance is, that they may become re-agents in disease, and contribute to the derangement of functions, whether by impairing mechanical relations which are natural, or some others, of dependence upon a material, which are acknowledged by all the organs engaged in the support of life.

CHAPTER II.

DOCTRINE OF SPASM.

In the more theoretical sciences, it often happens, that the career of improvement is marked by the substitution of a new error in place of an old one; by the refutation of one false doctrine, and by the proposition of another: and very fortunate may those who are engaged in the pursuit of such sciences think themselves if their labours are prosecuted in a stage when truths are attainable, upon which the spirit of inquiry might rest. In general, they must be satisfied with having suggested errors, like their predecessors, which may lead to more solid acquisitions at a happier period of research.

It was designed to supersede the mechanical philosophy in medicine, by doctrines, in which the function of the nerves was principally regarded.

The agency of these organs in disease was first insisted upon by Willis, who was followed by Hoffman, and many others, from the first proposition of the doctrine, to the present time. But it was chiefly by the opposition of Cullen, that medicine was rescued in the schools, from the precepts of the humoral pathology. Systems are seldom reformed in part. Since the time of Cullen, the "humoral pathology" has been little better than a term of reproach; and it is now much more creditable to speak about motions, and sensations, and sensibilities, and irritabilities, and irritations,

without attaching any very clear meaning to these words, than to speak of humours, salt, acrid, or bland; errors of place, or remoræ. After the rejection of the mechanical doctrines in medicine, the agency of the properties which distinguish the living from the dead fibre was particularly insisted upon. The influence of this reformation of the doctrine upon the practice of medicine, was rather to inculcate the rejection of the old remedies, which were employed upon mechanical views, than to propose new ones. The doctrine of Cullen could scarcely suggest any but a negative improvement in practice. The correction of the humours was no longer the business of the physician. But granting him to be instructed in the existence of a nervous influence, or of certain properties of life, however denominated, which were to be taken into the account, how was he to fashion a practice to the relations of properties, of the nature of which he was wholly ignorant? Doctrines of a practical sort have since been founded upon inferences respecting the existence of properties of life; but the worst of them are false and absurd; and the best of them, as we shall hereafter see, are very little the better, without the instruction of experience, which is, in some respects, empirical, for being derived from so refined a source.

Besides a general reference to the living state, which was to be considered in pathological reasonings, a particular exertion of the properties which distinguish this state was insisted upon by Cullen, and proposed extensively, as a solution of the phenomena of disease. This particular

exertion of the properties of the living fibre was chiefly exemplified in fever, which was thought to be essentially dependent upon spasm of the extreme vessels, occasioned by cold. The effects of this state of spasm were to suspend perspiration, to occasion heat of skin, and the other symptoms of fever. Granting, for the present, that the extreme vessels are liable to this state of spasm, the effects which are ascribed to it cannot take place without recurring, at least in part, to the humoral pathology, which this doctrine was designed to supersede.

The doctrine supposes, as fluids cease to be duly discharged by the skin, that there is a retention of them within the system, and that the effects, which constitute the symptoms of fever, are produced by this suppressed perspiration. The symptoms of fever are heat of skin, accelerated action of the heart, and perhaps invariably a local seat of inflammatory disease; thirst, furred tongue, scanty secretion of urine, &c. If the suppressed perspiration produces these symptoms, it can have such an operation only in one or both of two ways; first, either by the determination of a larger quantity of fluids to the internal parts, in consequence of their escape from the surface being prevented; or, secondly, by the retention of fluids which possess qualities of a noxious kind, and the evacuation of which by the skin is therefore necessary to health. The first alternative evidently proposes a mechanical explanation; and the second is nothing less than a recurrence to the doctrine of peccant humours.

That the mere suppression of perspiration is a cause sufficient to produce fever, is an opinion which is generally admitted. The correctness of it will however be doubted, when we consider the frequency of the occurrence. It happens to most individuals, perhaps every day during the summer, in a greater or less degree; and is strikingly the case with those whose occupations subject them to sudden and extreme transitions of temperature; from the forge, the glass-furnace, the bakehouse, &c. to an atmosphere perhaps below thirty. But it will be remarked in these instances, the perspiration suffers only a temporary check; it returns with the recurrence of labour, or with the exposure to a higher temperature. In many of those examples of disease also, which are ascribed to suppressed perspiration, the phenomena of the disease continue after the perspiration has been restored, even more copiously than before. This is particularly observable in acute rheumatism, in pneumonia, and in some local inflammations connected with the puerperal state, which are ascribed to cold, as inflammation of the breasts, legs, and even peritonitis: in all which cases, I have seen, no doubt in common with many others, patients appearing as if boiled in sweat, while the disease has run a protracted course, perhaps, though rarely, to a fatal termination.

It is, however, consonant with analogy, and with all that is known of the laws of the circulation, to suppose, that if perspiration is checked, and the vessels of the skin are in such a state as not to admit and evacuate their natural quantity of

fluid, there will be in consequence a vicarious determination to other organs. And although perspiration may in these cases be inadequate to arrest processes of disease which its suppression had originated, yet its restoration is always a favourable circumstance, if not one that is essential to recovery.

But the production of the phenomena which have been ascribed to checked perspiration, is liable to the alternatives of two modes; perhaps a third might be added. If the entrance of fluids into the extensive system of cutaneous vessels is resisted by a preternatural contraction of these vessels, it is obvious that those vessels which are not cutaneous must contain more than their natural proportion of fluids: this state would be one of general internal plethora. But we observe that, as a consequence of suppressed perspiration, one organ always suffers principally; that there is a particular determination to one seat. If a contracted state of the cutaneous vessels could throw an unnatural proportion of fluids into the internal ones, so preternatural dilatation of the vessels of any other seat or system would deprive the cutaneous vessels of their natural proportion. We have then to choose whether disease is maintained by suppressed perspiration, or whether the suppression of perspiration is a consequence of disease.

If we consider here merely the order of occurrence, it would appear that the disease, say, for example, acute rheumatism—was produced by suppressed perspiration. At the same time, there is no reason why the muscles, rather than any

other parts, should suffer from the suppression of perspiration; seeing that the simple circumstance of a diminished circulation in the skin is equally relative to the vascular system of every seat. In order that one seat should suffer a vicarious determination rather than another, it is necessary to suppose a predisposition in such seat which does not exist in others. This predisposition is very nearly allied to disease; and the admission of it goes near to confessing that the circumstance of a suppressed perspiration was preceded by processes of disease in that seat which afterwards exhibits its chief phenomena.

From this view of the subject it would appear, that the diseases which are imputed to a contraction of the skin by cold, precede the circumstance to which their origin is attributed; that the operation of cold is to determine the occurrence of symptoms which would be developed by spontaneous processes of disease which had already commenced. It is, however, probable, that if the processes had been entirely spontaneous, the form of the disease might have been modified by running into another stage of change before symptoms were declared. A woman who is menstruating, may at one time get wet in the feet, or elsewhere, and though the exposure to wet and cold may at this time be for a considerable period, she may in consequence suffer no inconvenience. At another time, when the same cause operates in a less degree, fever might speedily occur, with a local inflammation of the lungs. The disposition to this disease of the lungs must have preceded the ex-

posure to cold, and the disease in the lungs will run perhaps a protracted course, may terminate in phthisis, and may substitute the discharge from the uterus, the vessels of which, but for this vicarious determination, might have recovered a disposition, which was only transiently suspended, to pour out their natural periodical secretion.

If a disposition to disease in the organ which is afterwards the principal seat of it precedes the exposure to cold, the supposition of a state of spasm of the cutaneous vessels is very short of explaining *the origin* of disease: and that a predisposition in organs to disease does anticipate the influence of cold, is in agreement with all the facts which can be quoted on this point.

We find that the effect of exposure to cold is in some persons to produce rheumatism; in others, to produce gout; in others, to produce catarrh, affecting chiefly the vessels of the head, and the membrane of the nose; and in others, to produce catarrhal affection of the bronchia; in others, sore throat; in others, inflammation of the lungs; in others, derangement of the liver; in others, diarrhœa; in others, temporary diabetes, &c. The diminished circulation in the skin has no particular relation with any one of these organs, or seats of affection, but holds the same relation with respect to them all. If, then, one assumes in consequence a state of disease, it must happen from its being already predisposed to such disease; and that the subsequent course of the disease is maintained by the disposition of that part which is the seat of it, is proved by the circumstances, 1st. that

the cause of constriction of the cutaneous vessels, (viz. cold,) has ceased to act: 2d. that this constriction may have been in all these diseases overcome, which is proved by the occurrence of free perspiration: 3d. that when this constriction is thus overcome, the disease continuing, and the former cause (viz. cold) not operating (for the patient may be in bed), there is no reason why the constricted state of the cutaneous vessels should be resumed; or, if it is resumed, a causative relation subsisting between it and the disease, the disease is then the precursor of the constriction of the cutaneous vessels, and may therefore be as legitimately inferred to be the cause of this state, as the disease originally was, to be the effect of exposure to cold.

In addition to the experience of these relations just adverted to, it may be observed, that as the mode in which a constricted state of the cutaneous vessels is supposed to operate, is by increasing the volume of fluids in vessels which are not cutaneous, so the effects of this cause are obviated by the first depletion, which may perhaps consist of the loss of twenty ounces of blood, together with an active purgative. The contents of the vessels of the system generally, after such depletion, will be less than before the exposure to cold; if, then, it is to the increase of fluids internally that the disease is to be ascribed, how happens it that the disease continues when this quantity is diminished, as it is by a long treatment, greatly below that which belonged to the condition of health?

Although it cannot be denied that the cutaneous

vessels may be constricted by cold, and in this way produce some change in the balance of the circulation, yet such a change is totally inadequate to explain either the nature or duration of the disease which might succeed to it. It is necessary for the origin of such disease, that its processes should have commenced before the occurrence of the alleged cause; and its continuance, together with all the irregularities and changes which mark its course, are dependent, not upon a condition of cutaneous vessels, which is temporary, but upon the disposition of its seat; and this disposition is the result of modified relations of the properties of life, both among themselves, and in regard to their mechanical and hydraulic connexions.

It was suggested that certain effects of suppressed perspiration might be produced in two ways, both of which were a recurrence to the humoral pathology. The first was by a vicarious determination, or hydraulic process, which we have discussed; the second assumes that the matter of perspiration is of a noxious quality, and that its suppression is followed by fever and other consequences, which ensue from the operation of a morbific matter.

It is perhaps sufficient that the phenomena can be otherwise accounted for, from recognised agents, to render an inquiry concerning this latter mode superfluous. The occurrence of disease, however, by a causation of this kind, is not without the apparent support of facts. In the contagious diseases, their communication from one subject to another is most probably by means of

perspiration; many diseases appear to be thrown off, as is said, by perspiration; and that the body might generate morbid poisons (an expression which, in modern times, is preferred to morbific matter, although it is perhaps less correct), is proved least equivocally by some vaginal secretions which are capable of producing gonorrhœa, or pseudo-syphilitic sores, in those of a certain predisposition, who happen to be exposed to their influence. This is a case of the origin of a morbid poison, which may have only a temporary existence in the subject in whom it was generated, but may be perpetuated as a contagious disease, by successive communications from one person to another. This example, of the generation of a morbid poison, is perhaps the least equivocal; yet there are many others which it would require a pretty violent scepticism to doubt.

Certain infectious diseases, as the small pox, measles, &c. sometimes occur, where, from local circumstances, the possibility of communication with infected persons has been precluded. It will perhaps be said, the infection was conveyed by the atmosphere; this, however, appears very doubtful, when we consider that there might be no person suffering under the disease for many miles from the place where it appears, and when also it seems probable, from the observations of Dr. Haygarth, that atmospherical dilution so completely weakens the properties of infectious effluvia, that a very short distance from the person suffering under a disease of this nature, even in the same room, is sufficient to protect an attendant against

the risk of infection. I have known typhous fever, also, commence in a district, in a solitary and detached habitation, and the first subject of it has been a child, who certainly never travelled so far as to a neighbourhood in which the disease had already appeared. In addition, therefore, to the unequivocal origin of a morbid poison, quoted in the instance of the vaginal secretions, it is difficult to doubt but that contagious and infectious diseases might be generated in bodies which have undergone preparatory processes of disease, which have not before gone beyond the limits of predisposition. At the same time, it is probable that a peculiar state of atmosphere may be connected either with the history of this predisposition, or that it might have the relation of what is termed an exciting cause. This is probably the case in many instances: in typhus, from the highly fœtid state of the alvine discharges which have for some time preceded this disease, it has been the suggestion of reiterated experience that the source of the infection was, in the instances alluded to, in the bowels; and that the symptoms were produced by the absorption of fluids, charged with noxious effluvia.

The tendency of my own observation, if it does not inculcate the belief that perspiration may possess the quality of originating disease, certainly presents some analogies which will not permit us to conclude against its possibility. But it does not appear that the subject, in whom a perspiration of this quality may be generated, is likely to get a disease, in this way, from its suppression. When

perspiration is suppressed by exposure to cold, there is for the present only a suspended secretion; under this circumstance, the subject is not more liable to disease from retention of perspirable matter of a noxious quality, than if he had not been perspiring at the time of exposure to wet or cold. As fever rarely happens without being preceded by checked perspiration, so perspiration, and then exposure to cold, seem necessary to the disease; which would not be the case, if the mere retention of perspirable matter of a certain quality were capable of producing it; for then the absence of perspiration would be equivalent to its being suddenly checked. Indeed, as the quantity of infectious matter deposited on the skin by perspiration is incomparably greater than that which might be obtained by the mere contact of individuals, so the chances are rather in favour of a person's infecting himself, by the absorption of noxious qualities from his own perspiration, if these existed, without the aid of cold, than by a mere contact with the skin or clothes of another, which, in the contagious diseases, is the common mode of communication.

There is another effect which has been imputed to spasm, the consideration of which belongs equally to a future part of our subject. It has been attempted to explain the hydraulic phenomena of inflammation, by the agency of spasm affecting the vessels of its seat. The theory is, that certain vessels are, in inflammation, obstructed by spasm; and that blood therefore accumulates in vessels so circumstanced, and is determined into other vessels,

by which a general dilatation is produced, and a corresponding tumefaction of the inflamed structure, in consequence of its possessing, by this mode, a preternatural quantity of blood.

This theory must assume either, 1st. that the arteries, or 2d. that the veins, or 3d. that the capillaries of the seat of inflammation are affected by spasm. And the subdivision of alternatives is, that it is to this condition either of one or more vessels, of either order, that the tumefaction is to be attributed. We will take as our example an inflammation of a finger, from a puncture under the nail.

It is perfectly obvious that the digital arteries are not, in this case, affected by a spasm which impedes the circulation; for the pulse in these arteries may be felt, at the extremity of the finger, with more than ordinary distinctness. If, then, it is neither trunk of the digital arteries, does a state of spasm prevail in their ramifications? If this state of spasm affected the minute arterial branches, the effect would be, that the seats to which they are distributed would be deprived of blood: in agreement with the more palpable deprivation of this fluid in other instances, there should be a trifling diminution of temperature, with sense of numbness; but these are not the consequences of a wound, which is followed by inflammation.

But supposing that certain arterial branches of a seat, or all of them, were affected by spasm, the effect upon the circulation would be very trifling; for the blood which would have been distributed by these vessels, finding the freedom of its trans-

mission opposed in one limited system of branches, would be disposed of in the trunks, and in the branches which were nearer the heart. During the erection of the penis, the volume of blood in its vessels is greatly increased; yet when these vessels resume the state by which their capacity for blood is diminished, the quantity which, in the dilated state of the organ, would be contained in its vessels, is readily disposed of in the superior system of its arteries. The conclusion that a similar adaptation obtains generally, is, I believe, in agreement with every palpable analogy with which our experience furnishes us.

To suppose that tumefaction may be caused by spasm affecting the veins, would be to attribute to this structure an action of which we have no reason to believe it capable. But supposing a venous trunk to be so constricted by spasm, as to impede totally the passage of blood through it; this might even be the state of one or two much larger veins than those of the seat, which constitutes our example, without either tumefaction or pain. If the blood cannot be returned freely by one vein, it returns by another; and if the impediment should be a general or more diffused one, owing to a spasmodic state of many branches of veins, this impediment, cæteris paribus, would be obviated by the ready disposal of a trifling increase of the quantity of blood in the superior ramifications of the arteries, where no such impediment existed. It is one law of the circulation, that the blood flows in the largest quantity, where it is least resisted. There is also another provision against the

occurrence of a congestion from such a state of the veins as that which we are considering; viz. that if the transmission of blood through an artery is prevented, as by a ligature (which is quite as effectual as spasm affecting the origins of veins), the blood is disposed of, without the assistance of veins, by anastamosing arteries. And so readily does the circulation accommodate itself to an impeded transmission through one vessel, by adopting another course, that it might even become retrograde, from the increased pressure of blood on the vessels inosculating with that which is impeded, and the facility of distribution in the system of that which is unimpeded. If this latter resource should fail, the circulation is no longer carried on; the blood is at rest, and it coagulates, and the circulation proceeds only where there is freedom of space and facility of communication. As this account, which agrees with all our experience, is drawn from instances in which the impeded circulation is both of a more decided kind, and in vessels of a much larger order; such as those which are the seats of aneurism, and of the employment of the ligature; it is to be presumed that a causation which obtains in the least degree is not likely to produce effects which do not arise from a similar causation, when the degree is infinitely more considerable.

If a spasmodic state of the arterial or venous structure of an inflamed part appears inadequate to explain the circumstance which has been imputed to it, we are not likely to find a sufficient cause of plethora and tumefaction in a similar state of vessels of less importance. We need

scarcely inquire whether spasm is a possible or frequent occurrence in the capillary system: this seems superfluous, so far as the present question is concerned. For, if the resources of the organs of circulation are capable of providing for the most considerable obstructions of the blood, it is scarcely probable that the same resources should fail when the obstruction respects a fluid which is less important than blood, and which is only a separation from it.

Thus it appears, that a spasmodic state of any order of the vascular system is inadequate to account for the increased volume of blood which is contained in the vessels of an inflamed seat. The idea of a spasmodic state of the vascular system, at any time, or under any circumstances, except perhaps some of unnatural exposure or direct mechanical agency, appears to be little better than a pathological dream; for the conclusion is supported by no direct testimony of experience, and the evidence of analogy is equivocal and remote. It remains, however, that we should examine from what sources this evidence is derived.

If the circulation through the veins of the arm is prevented by a ligature, as in the common operation of bleeding, the vessels below the ligature become enlarged; the same degree of constriction being continued, either the internal veins would assume a dilatation equal to the return of the blood conveyed into the fore arm by the arteries; or if the ligature were very tight, so as to impede also the circulation in the deeper seated veins, the fore arm might swell, perhaps not immo-

derately, and mortify. I have reason to think the power of accommodation in the deeper seated veins very considerable under these circumstances. It has occurred to me to tie up an arm for the purpose of bleeding. This operation has been resisted, from a violent antipathy to it; and I have persevered in solicitation and entreaty to suffer me to open a vein for three quarters of an hour before the patient would consent, during which time the arm remained tied up in the ordinary degree of tightness; and at the end of this period, although the ligature did not appear to have been relaxed, the distention of the superficial veins was so much less than immediately after its application, that it was necessary to tighten it in a still greater degree before puncturing the vein.

The example of the enlargement of the veins below a ligature furnishes an analogy, by which it is concluded that a spasmodic state of these vessels might produce a similar accumulation of blood. It is indeed quite natural to expect, if blood is poured into the arm by the arteries, and its return prevented by the veins, that vascular distention would take place in consequence. But in this case, it is not an obstruction of one, or two, or a dozen branches of veins, but of the venous trunks; an affair of much more importance than such a venous obstruction, as might be conjectured, according to the theory, to occur in an inflamed finger. It would not be expected that the circulation should still be performed where the return of blood from a whole limb is prevented by mechanical pressure on its principal veins: but it may be

conjectured, that when the obstruction is very limited and partial, the blood may find its way back to the heart without much difficulty; seeing that the obstructed vessels, compared with those which are unobstructed, instead of being, as in the fact which furnishes the analogy, perhaps as three hundred to one, are as one to three hundred.

To consider a little further the facts connected with this analogy: we find that considerable trunks of veins are for a long period obstructed by the pressure of the gravid uterus; and though the obstruction is pretty considerable, it is no part of the operation of such pressure to produce any thing like the phenomena exhibited by an inflamed finger, from a puncture under the nail.

But, if it be objected, that the pressure in this case is on the trunks, and that the spasmodic state is supposed to affect the origins of veins, we are not without experience that an impeded circulation in this order of vessels is not productive of inflammation, or is readily accommodated by the provisions for such a state. Thus, a person might sit for many hours in one position, in which there must be an extensive and effectual compression of the superficial vessels; yet this does not produce inflammation. Or, to take a stronger case: a person, with a compound fracture, might preserve one position, almost without moving, for a week or more; perhaps, without an interruption of pressure on some parts for many weeks; but for one week uninterruptedly the cutaneous vessels on one side of the body may be compressed; yet we find no swelling, nor is there any reason to think that the

blood which should have been distributed on so extensive a surface is not very readily disposed of in communicating systems of vessels. The same conclusion is also supported by the effects of bandages.

It is quoted as a very conclusive proof of the mode in which the swelling of inflammation is produced by obstructed vessels, that if the circulation is impeded in one or more vessels in the web of a frog's foot, such vessels become enlarged, and the web exhibits the appearance of inflammation. This may be done by a ligature, or by compressing the web with the blunt edge of any instrument, or hard substance. This fact would indeed appear conclusive of the possibility of inflammation supervening by a process of this kind; but the experiment is not simple enough for this purpose. The obliteration of the vessel cannot be made without mechanical injury, which injury is a cause of irritation; and it is to this irritation that inflammation is in other instances ascribed. If irritation is sufficient alone to produce inflammation, this effect cannot surely be imputed to an influence which is merely an accidental accompaniment to the common cause of inflammation. The question is, how irritation produces inflammation? and if the means of irritation happen to be of a sort which also impede the circulation, we are not to ascribe to the latter an effect which the former is adequate to produce: we are no more to give this importance to the accompaniment in question, than to the colour of

the thread of which the ligature consists, the kind of metal with which the web may be compressed, or to the gloves of the experimentalist, if he should happen to wear any.

The effect of the ligature upon an artery in the human subject, or in other animals, is not to produce immediately an increased distention on the side nearest the heart. I have never remarked such a distention to follow the application of a ligature upon arteries which have been tied in the operations of surgery, or in rabbits, which have sustained experiments. On the contrary, it appears, according to the observations of Dr. Parry, that the circumference of an artery has suffered a trifling diminution of $\frac{3}{400}$ of an inch, on the side nearest the heart, after the application of a ligature*. If time had been allowed for inflammation to supervene, in all probability the vessel would have been found considerably enlarged. It appears, therefore, more correct to refer the dilatation which is produced by compression of the vessels in the web of a frog's foot, to the greater delicacy of this structure, the sensibility or irritability of which disposes it to an immediate re-action of an inflammatory kind, as an effect of irritation, or under the infliction of an injury.

Against the supposition that vascular spasm is an effect of irritation, the direct fact is opposed that an artery will not contract on the application of a stimulus; that no chemical or mechanical

* Inquiry on the Arterial Pulse, Exper. xvii.

agents, as Dr. Parry remarks, "applied to any part of an artery will cause it to contract*."

If from these considerations it appears improbable that irritation is a cause of vascular contraction, it will be readily conceded, on the other side of the question, that irritation produces a state of an artery which is the reverse of contraction: the superficial arteries may be seen to undergo considerable dilatation, if the structures to which they are distributed are subjected to any adequate causes of irritation. It is also the effect of the irritation of an animal passion to produce considerable dilatation of the branches of the pudic artery: the distention which succeeds in the structure to which these branches are distributed, must be ascribed to the vascular dilatation which irritation produces; for a similar state of the structure would not be produced, or any thing like it, by a ligature, whether employed on the arteries, or by one which would completely obstruct as many veins as could be compressed, without at the same time compressing the arteries.

To say in agreement with what law of the physiology of the vascular system irritation should be capable of producing vascular dilatation, does not belong to this place: indeed, it is doubtful if this effect can be connected with any powers of the arteries hitherto recognised. Their dilatation from this cause has been ascribed, among other modes, to a loss of their contractile power; but the loss

* Inquiry on the Arterial Pulse, p. 52.

of a tonic, or contractile power, is not in agreement with the usual effects of irritation in a muscular structure, which certainly in general excites contraction. It appears probable, that this circumstance is connected with some laws of the circulation which are hitherto undeveloped. In our future considerations on this subject, under its appropriate title, the laws here alluded to, respecting the government of the circulation, will be distinctly suggested.

From this view of the subject, it appears that not only is the occurrence of spasm not supported by experimental evidence, but, on the contrary, that it is irreconcileable with all the evidence of this kind which can be quoted on the point. It also appears, even if the supervention of a general spasm of the cutaneous vessels from exposure to cold were conceded, or if it were allowed that the more limited one which has been supposed in local inflammation was ever produced by irritation, that, in neither case, is the cause adequate to the explanation of the alleged effects. I am not aware of a satisfactory proof, that any order of vessels is, at any time, or under any circumstances (unless some of preternatural exposure should form an exception), in a state of spasm.

It is a conceded point in the physiology of the arteries, that they possess a tonic contractile power, which may reduce their calibre to a less diameter than would ensue from their elasticity alone; but the diminished area of a vessel is one

thing, and the obliteration of it by spasm another. Considerable variety may be confessed in the diameter of the same vessel, at different times, owing to a variable contractility, which may be readily affected by external causes, or by internal changes: but under the most considerable degree of shrinking of the skin, which is produced by cold, under the cold fit of an ague, it is not to be doubted but that the minutest cutaneous vessels would bleed, if scratched with a lancet ever so superficially; our proofs go only the extent of demonstrating, that vessels are furnished with a power of contraction, which is variously exerted at different times; they furnish us with no example of a spasm, which would render the local circulation of the blood difficult or doubtful.

It has been mentioned, that in amputations performed in latitudes, and at a season of the year, when the thermometer has been some degrees below zero, it has been found necessary to tie none but the largest vessels. The property of cold in diminishing the area of arteries is one, the exertion of which is very familiar: but in the extremest cases, the facts go the extent of proving only a considerable reduction of their diameter, never their entire obliteration, by this cause alone, either on a limited or extensive scale, as long as they are liable to the influence of the circulation, and in circumstances not so totally preternatural as to preclude altogether the argument of analogy, for which these facts might be cited. But if the

supposed spasmodic state of the vessels should still be insisted upon, sufficient has been said to show that it rests upon defective proofs: and if this imperfection of proof should be supplied, that the phenomena imputed to it are still but inadequately explained.

CHAPTER III.

DOCTRINE OF BROWN.

The doctrine of John Brown was a production of genius; an offspring of imagination, but little corrected by the understanding: it was an intellectual meteor, the light of which was brilliant, but deceitful and evanescent. Such was its general character and destiny: a few rays have, however, survived, and still rescue the doctrine from entire oblivion.

The pathology of Brown was, perhaps, a more complete rejection of the notions about tubes, angles, humors, and particles, than any which preceded it. According to him, there existed in animals a principle, capacity, or faculty for the phenomena of life: but this capacity was imperfect of itself; it required, in order to produce the phenomena of life, that it should be subjected to influences derived from the external world. These influences were called stimuli, and they were said to consist in food and air—exercise was also added; but as this was an effect, or phenomenon of life, it could not well be considered as its cause. It may, however, be among the stimuli which influence life; but in this quality of an effect, is not necessary to be enumerated among its causes. The passions likewise are called stimuli; but these being also phenomena, or effects of the living state, cannot be quoted as

the causes which originate life. The character of stimuli is very capriciously assigned by the doctrine: "sadness, grief, terror, and despair," are said to be only lesser degrees of "gladness, confidence, and hope." Those, which in medicine also, are considered as sedative powers, are said to be *stimuli* of a weaker degree; hence, according to the doctrine, the means which suspend, or extinguish life altogether, are to be reckoned only as the weakest among its supporters; the argument, carried a little further, would show, with equal reason, that water is only a weak degree of fire. The capacity for life with which every animal was endowed was called "excitability:" between this excitability and the stimuli there was a relation, from which life resulted.

As this simple apparatus was all that was necessary to life; as life consisted but of two parts, the due proportions of which constituted health, it was perfectly certain, that if disease occurred it must be the fault of one of them. Accordingly their varieties are enumerated, chiefly with reference to their quantities, in which limitation it will appear hereafter there is an essential omission. Among these varieties, it is said, that when stimuli are in excess, the excitability being also pretty abundant, life burns (to use a metaphor which is common in this case) with too much intensity, it is in a state of preternatural vigour, and gives rise to a *sthenic* diathesis, which is the term by which this state is denoted. It is said also, that there may be a deficiency of stimuli, by which the flame of life will sink too low: this

is a state of *direct debility*—a consequence of this state is, that the excitability *accumulates*, not being exhausted with its usual rapidity by the operation of stimuli. The other variety (for there are only three) is that, from excess of stimuli, the flame of life (to continue the metaphor) also sinks too low, in consequence of an exhausted excitability, which ensues from the operation of an excess of stimuli: this is called a state of *indirect debility;* and both these states of debility are designated by the word "asthenic," as that of excess of vigour is by the term "sthenic," disease. Thus "the infinite malady" is reduced to three states, and of course all the varieties of disease are but varieties of these states.

Having so clearly demonstrated a simple pathology, it was to be expected that a system of therapeutics would be appended of equal simplicity, and in this expectation we are not disappointed. As the *sthenic state* was produced by excess of stimuli (or for which such an excess was indispensable), the method of cure was, of course, to diminish them: the means consisted in various degrees of depletion and abstinence. As the state of *direct debility* proceeded from deficiency of stimuli, the method of cure was to increase them: as the state of *indirect debility* arose commonly from an habitual excess of stimuli exhausting the excitability, this state was remedied by a gradual diminution of stimuli, which would permit a renovation of the excitability. With so simple a pathology, and so obvious and certain a method of cure, one would almost be surprised

that men should ever die. This doctrine consists of a physiology, and a pathology founded upon it.

With respect to the first part of the doctrine, its physiology, it has been asked with reason, in what manner the *excitability* is renewed? It is said that animals are endowed with a certain quantity of this principle: that it is undergoing a perpetual exhaustion. It is spoken of as being at advanced periods of existence in excess, from reduction of stimuli; at the same time, the quantity must actually be less than in the state of childhood, or infancy. It is spoken of as accumulating from this cause: and it is spoken of as attaining a healthy degree, after having lapsed into the state of indirect debility. This is a confession that some source is necessary, by which this principle may be accumulated under deficient stimulation; and by which it might be renovated after preternatural exhaustion. The doctrine thus far carries with it a refutation, of which other pathologists have justly availed themselves.

Had the author of the Brunonian system been aware of this difficulty, his doctrine must have been entirely abandoned, or modified in one of the two following ways: either, first, he must have supposed that the original quantum of excitability was undergoing a perpetual waste by the action of stimuli, without any means of renovation or accumulation, which would have been to have given up the curative doctrine of indirect debility; or, second, he must have admitted some source from which this principle was renewed, an admission which would have influenced the doc-

trine of relation between excitability and stimuli, in every instance.

To suppose that an animal was originally endowed with a fixed sum of a principle of life, which was afterwards to undergo perpetual and rapid exhaustion to the time of his death, is contrary to all analogy: it might be said that such an opinion is not merely irrational, but absurd. The supposition implies that there is one part (or set of properties) of life, for the renewal of which there is no source. It follows, if there is no source for the renewal of this principle, that the portion of it which now animates men has been identically derived from our first parents. It follows, that the quantity which is finite in the system generally, is next to infinite in that little aggregation of animal substance which we call the ovum. Now, if in the diffused textures of a human being, the quantity is fixed, and only equal to endure a few years, is it probable that such a being could confer on a rudiment of fœtal existence, not merely a sufficient quantum to endure through his future existence, but enough to confer a material of life which shall endure in perpetuity, and furnish all future generations?

There is palpable absurdity in the proposition that any part of the principle of life should be conferred as a fixed sum, upon the animal whose existence is afterwards to last only until this sum is consumed; and the absurdity of this proposition is the greater, as it is unsupported even by evidence of the weakest kind. The only semblance of testimony which can be quoted on its behalf is,

that disease is produced by excess of stimulation, and is perhaps cured by its reduction. But this proof carries with it the refutation of the doctrine; for the cure implies, that the principle which had been exhausted, under a diminution of stimuli, recovers itself, or is reproduced, for which, according to the theory of a fixed sum of life, there is no provision.

In cases in which no proposition can be established by arguments of analogy, we have no resource but to rest upon our experience: we can conclude beyond our experience only by the aid of analogy, and where this voice is silent our information stops. There are two sorts of conjectures; one is conjecturing that a thing is, and the other is conjecturing whether it is not. The question, whether one part of the principle of life might not be fixed, and the other renovated, I fear belongs to the latter class. The conjecture that a thing is, results from the most distant indications of analogy; the conjecture whether a thing is not, is a pure exercise of the fancy; as one might conjecture whether invisible fiddlers do not play to invisible dancing masters; whether the bodies of those who feed chiefly on vegetables, may not endure longer after death than the bodies of those who lived principally upon flesh; whether crocodiles do not sometimes become men, and men sometimes degenerate into crocodiles; whether cats were not created before dogs, &c. We should think of making but one answer to such vagaries of the imagination: to as many questions of this kind as could be proposed, we should say we must consult our experience.

Recurring to our experience on the point in question, we find that no one property or set of properties of life is less dependent upon the means of nutrition than another. The supporters of life are food and air; without these its phenomena cease: we have no proof that any properties remain which were not dependent upon these sources; or if it is conjectured whether any do not survive, which are independent of means of nutrition, we have no more reason to think that the surviving properties are of one kind than of another. We have no proof that any are independent of the supply of food and air, in a ratio to their exhaustion, or rather their conversion into another form of being: but, if we had proofs on this head with respect to some properties, we have no evidence to determine our selection in favour of any in particular.

But to consider the argument of analogy, to which we have slightly appealed. The only analogy which can be quoted, is that founded on the similitude between properties of life; and as we have an experience of the dependence of some properties of life on the means of nutrition, we must infer a similar dependence of all the properties of life upon the same sources, until it shall be shown that there are some properties of life, which are not subject to the laws which are common to all others. But that the properties of life are all alike renewed from the sources of air and food, is in agreement with every analogy both near and remote with which our experience furnishes us: that certain properties are undergoing perpetual exhaustion, are never renewed, yet are never ex-

hausted (for this, according to the doctrine, is the fact, tracing the distribution of this supposed principle from its primitive sources) is as contrary, as the converse is agreeable to our experience.

The whole of our information with respect to the terms on which life is maintained amounts but to this; that all the properties of life, in conjunction, produce the phenomena of this state; that the existence of these properties is inferred only from their phenomena; that the cessation of the phenomena, by parity of reasoning, gives rise to the inference that the properties of life also have ceased to be, or have changed their form; that is, all the properties of life are concerned in its phenomena; so an inference made from these phenomena which is true of some, is true of all the properties of life *; that as life is constantly engaged in actions, or producing a succession of effects, so it is to be inferred, that successive quantities of this entire principle must be provided, in order that the succession of causes might be equal to the succession of effects. This is in agreement with a doctrine of causation elsewhere stated †, which it is presumed, on physical testimony, is irrefragable; lastly, that the continuance of the

* Some exception might appear to be furnished by the muscular structures; in which the property of contraction survives apparently the others. But this might happen because the effect of a property of contraction which is nearly extinct may be still witnessed by the senses. Other properties may also be in a similar stage of extinction, but are unequal in this state to produce perceptible phenomena.

† Indications which relate to the laws of organic life.

living state requires the formal or living condition of the properties of life, which renew themselves, by assimilation, from arterial blood. This process of assimilation has been elsewhere* stated to be of the following kind: namely, that arterial blood possesses life in an informal or elementary state; the relation of life, with the material from whence it is renewed, in every animal, is with its own similitude; the influence of life is, to separate from arterial blood those elements which constitute its own identity, and thus it lives, by repeating the same process, on other elements, in proportion as it is exhausted, or passes away in the phenomena of which its properties are the causes.

As we find that the physiology of the doctrine which we are considering is false, of course the pathology, being founded upon it, must fall to the ground. But as the physiology is in some measure deduced in conformity with a pathology, it remains that we should examine this pathology also, and not refuse to either of these doctrines all the support which is to be derived from the other.

Diseases, according to this pathology, are only of two kinds, one arising from excess, the other from deficiency of vigour. The state of life is otherwise designated by the term "excitement;" and the varieties are said to consist of too much, or too little, an increased or diminished excitement. Thus it appears, that all diseases are attributed to the *degree* of excitement.

The doctrine, unhappily, does not propose a

* Indications, &c.

criterion by which the degree of excitement might be judged of; it makes, however, an arbitrary assignment of certain diseases to certain supposed degrees of excitement; but as we cannot say to what degree of excitement certain phenomena are to be ascribed, without some means of knowing this degree, so we must suppose a criterion, which the author has neglected to do, in some function, either of the animal or organic system.

If the strength of a subject is judged of by his muscular power, we find that this may be the least when the sthenic diathesis prevails; as, in forty-eight hours after the commencement of an inflammatory disease, the subject who could before walk perhaps thirty miles in a day, can scarcely walk across the room. Depletion, according to the doctrine, is the remedy for this state of disease; the effect of depletion is to produce debility, yet the debility in the animal system is already excessive; the animal system, then, does not appear to furnish the criterion of strength or weakness.

In the organic system, it would be difficult to find any one state, or definable collection of symptoms, which is not common both to the sthenic and asthenic classes of disease. The rate of the pulse may be the same in a robust man, in the commencement of pneumonia, as in one in the last stage of consumption, who has suffered a chronic course of depletion and abstinence, and from whom the means of strength have been for perhaps many months withheld. It is equally obvious that no other state of the pulse can be assigned, which is not common to diseases whose history and charac-

ter are totally different. The same remark is true, if the criterion is looked for in any other organ or function: thus the appetite may be the same in health, and under the diseases of cancer or consumption; the urine may be as copious with a schirrhous liver, as if the liver were not diseased, or as scanty in dropsy as in typhous fever; or in some forms and stages of these diseases, neither more copious nor scanty than in a state of health. Perspiration also may be abundant both in health and disease, and the tongue more furred every morning in those who get up to go about their daily labour, than in a person who is suffering under chronic rheumatism, with a pulse of a hundred and ten, and buffed blood.

Will it be said the criterion is derived less from any actual symptom of disease, or condition of an organ, than from the previous habits of life of the subject? that if his means of nutrition have been scanty, or of indifferent quality, the diseases of such a person are those of debility? that if his diet has been abundant and nutritious, his diseases will be those of the sthenic diathesis?

This criterion would indeed afford as much facility in practice as one could reasonably desire; but as we find that those, whose means of nutrition are limited in quantity, and of a quality not to be boasted, are often the strongest men, and the subjects of inflammatory diseases of the greatest violence; and, that those whose means of repletion are the most ample, and whose habits of living are such as should confer the highest degree of health, are, on the contrary, frequently weak, incapable of

much bodily fatigue, not subject to violent inflammatory diseases, and soon reduced by treatment to extreme prostration of strength, or even faintness; as these results are so little consistent with the alleged tendency of the means, it seems probable that the tendency is falsely or imperfectly deduced, and that a criterion of strength or debility is not to be derived from this source.

Without troubling ourselves to look any further, it might be safely affirmed that there is no degree of life, so far as degree may be judged by the energy or debility with which any one or more function is performed, which does not obtain in diseases, different, perhaps opposite, in their general character. If then different, or opposite states of disease, confessedly requiring opposite treatments, might exist with the same degree of excitement; so far as this can be judged of by the energy or debility of function, it is obvious that the causes of diversity of diseases are not to be found in analogous circumstances, but in those in which the respective examples differ from each other.

It is a proposition to which assent would at first sight be yielded, that, as the state of health is a precise one, though with difficulty defined, and as strength appears to consist in the due performance of the functions, both of the organic and animal systems, which is a result of this state of health; so the *general consequence* of a deviation from this state, or of disease, must be debility: more briefly stated—as strength depends upon one state of the related agents of life, which we call health, so de-

bility must always ensue from a deviation from this state. The conclusion is perfectly true, if the proposition is true on which it is founded: on examination, this will not be found to be precisely the case.

In fact, vital energy is not so considerable in that state which we call health, but that the respective functions to which it subserves may be performed with a greater degree of strength, if this degree may be measured by the effect of the function. This increase of the function, under many circumstances, may be preternatural, and will therefore be regarded as an effect of disease: thus the function of secretion in the kidnies is increased in diabetes; thus the function of secretion in the vagina is increased in fluor albus; that of the mucous membrane of the bowels, in some cases of diarrhœa; the power of action which animates the heart, is increased in all fevers, in consumption, &c. thus, also, the secretion of the salivary glands is increased by mercury or by a catarrh; that of urine, by the employment of diuretics; the powers of the brain, perhaps, by wine, æther, laudanum, &c.

Thus it appears, that the highest degree of energy of functions is not the result of health, nor is it the invariable effect of a deviation from this state to produce debility: on the contrary, it is the effect of some preternatural influences to increase, and of others to diminish the degree of functional powers.

But there is this difference in the relation of these sets of agents; that under an influence which increases functional power, the nature of that

power might be preserved, or may be unchanged: the effect may only respect its degree; while that which *diminishes* functional power, may do it either directly, as related with its degree, or by an operation which would be common to all causes not directly related with its degree; namely, by changing its nature, in proportion to which the power would be weakened, until the change of the function, perhaps a progressive one, ended in death.

As the question with respect to the degree of excitement, or more definitely, the power with which functions are exercised, is one which is perpetually recurred to in medical reasonings, it appears admissible, in this place, to extend a little our discussion on this point; and this seems the more desirable, as I am not aware that the consideration has ever been fully gone into. We will take any specimen of increased function as an example; to define one, we will say, that the degree of excitement, which actuates the heart, is greater when this organ beats a hundred and thirty, than when it beats seventy strokes in a minute.

Our inference that the degree of the power of action is greater in the former than in the latter case, is made from analogy to things which afford a sensible test; the effect of a power is increased, and therefore we infer, agreeably with our experience, an increase of the cause or power itself. Thus, certain substances may be fused slowly or imperfectly by a weak fire, and rapidly and completely by a fire which is more intense. To understand this matter, it is necessary that we should go into the general question, on the dependence of degree.

Degree differs from quantity in this respect, that by increase of degree we mean an increase of intensity: by increase of quantity, we do not mean an increase of intensity, but merely a larger bulk of a thing, the intensity of which may remain the same. This distinction is exemplified in chemical substances, which may receive great additions to their bulk, without a greater intensity of any one property. The fire also, produced from substances which are of slow and imperfect combustion, in point of quantity, may greatly exceed that, in any given space, of a smelting or glass furnace, yet the degree or intensity will be considerably less.

Thus far the distinction appears to be merely the relation of a common fact; but it is to be asked, what is our experience of the causation of degrees, where the cause and the effect are subjected to plain experiment, and to the evidence of the senses?

We find that a dram of a concentrated acid will produce a certain degree of acidity in a given quantity of water; that two drams will double this degree of acidity, &c. But if an equal quantity of water be added to our given one, also containing a dram of the acid, the bulk, or quantity of acid, is increased, but not the degree. The same thing may be remarked with respect to heat, which admits a thermometrical measurement.

Hence it appears, that there is in substances, which are liable to be considered in reference to their degree, a property, the degree of which is contemplated either by its sensible or inferred effects—that this property has a perfect or un-

mixed state, and that its degrees depend upon its dilution. The relation then between quantity and degree is of this kind: the degree depends upon the comparative quantity of the property in question, to the quantity of substance of another kind, with which it is mixed. Thus much for the *dependence* of degree: the relations of *degree* and *quantity* remain to be distinguished.

A certain degree of a property is capable of producing, in relation with other substances, a certain effect; an increase of degree will not always produce an increase of the same effect; on the contrary, it destroys this effect or produces another. In other instances, an increase of degree produces an increase of the effect: an example of the first proposition is, that one degree of heat promotes life, a greater destroys it: there are many more examples of the same kind: an example of the second is, that one degree of contractile power in a muscle may be equal to a weight of twenty pounds, and a greater degree equal to a weight of fifty pounds.

The conclusion from these facts is that the influence of degree, in producing effects, is the common one of all causes: that a substance is precisely identified by one degree, and, in relation with other substances, produces the precise effect which belongs to its constitution, as three and two make five: the degree altered, its relation with the same substance is altered, and consequently its effect; and for the same reason as *three* and *two* make *five*, *six* and *two* make *eight*.

But it is a result of increase of degree, as stated

in the second proposition, sometimes to increase also the degree of the effect. In this case, the relation of degree with the other causes which help to constitute the effect, is not to produce an effect in which the resemblance to that produced by a weaker degree will not be recognized; but to modify an effect, which, under this variety of causation, preserves its nature with increased force, or intensity; which increase of force or intensity of the effect, in these latter instances, become a measure of a corresponding increase in the degree of the cause. If we are asked how this seeming diversity is to be explained? the question admits of no answer, but such as is comprised in a statement of the fact. We cannot explain why any particular form of existence should be that form, and no other; we can only define relations, the study of which constitutes the business of philosophy: we cannot say why the causes of a muscle should be a muscle, or why an acid and an alkali should be a neutral salt; we refer to their existence, and say such they are. With existences we work; and with respect to their forms, we aspire to distinguish, to arrange, to separate, and to combine; but we can give no *à priori* reason why they should be as we find them, or why they should be otherwise than they are. We can also demonstrate, to some extent, the condition of their existence, which is that they are identified in these forms, by other forms of existence which they comprise, from the combination of which they originated, and on the continuance of which unmixed combination they depend.

This has been elsewhere explained, at sufficient length*.

It appears, then, that the relation of degree in causation is to produce a certain effect, in conjunction with the agents with which it is related; that an increase of the degree sometimes produces an increase in this effect, and sometimes destroys the effect altogether.

It appears that the relation of quantity in causation, respects the repetition of the effects which depend upon degree. There are cases in which these agencies are so complicated, that it will be difficult to distinguish them. This, however, in order to gain a consistent doctrine, must be attempted, as well as a discrimination of the cases in which the effects of degree are preserved under an increase of it, and those in which they are lost. Our discussion on this subject of degree, has reference to the force or power of those properties of life which animate the systems, and are chiefly instrumental to the functions of living bodies.

If the heart contracts during health, to state a supposititious proportion, with a force equal to ten pounds, and during disease with a force equal to fifteen pounds, we should conclude that the degree of contractile power was greater in the latter than in the former case. We have seen, that in our sensible examples, the degree of a property depends upon the comparative quantity of the

* Indications, &c.

property with others, with which it might be mixed or combined. As the degree of contractile power, in the case we are considering, is increased, it seems necessary to suppose that a change has taken place in the source of this power, by which a preternatural quantity of the property is produced.

Such a conclusion appears very obvious; yet the inference is not so clear and simple, but that our discrimination may be exercised even on this point. The degree of a property may be increased by an increase of its quantity in the medium with which it acts; or without any increase in the quantity of a property, its degree may be increased, as in our sensible instances, by a diminution of the substance, or medium by which it is diluted. In the causation, therefore, we have to distinguish between two possible modes—a distinction which cannot be made it is presumed in this stage of analysis.

If there is a property in the liver which separates bile from blood, are we to infer from an increased secretion of bile, that the degree of this property is increased? This is one of the cases in which the distinction between the effects of degree and quantity is not easily made; for an increased quantity of the secretion might be a result of increased degree, or an increased degree, if possible in this instance, might have no such relation: the increased quantity of the secretion might result from an increased quantity of the principle which separates it, in its ordinary degree of strength or dilution. The same may

be observed of all the other secretions; of the formation of fat, of the general increase of the structures.

But, if there is difficulty in making the precise distinction just suggested, there is none in attaining the more important conclusion; that, whichever of these alternatives is adopted respecting the mode in which increased results of functions are produced, the disposition to such increased products is only one phenomenon of a causation among properties, the nature of which is not the better understood because it is expressed only by the degree or quantity of its effects.

If we have found the conception of the *increase* of degree to be attended with some difficulties, these will, I fear, be multiplied as the alternatives are multiplied, by connecting with our former views the causation by which the force or degree of properties might be *diminished.*

Recurring to sensible examples, we have seen that substances, containing a property, the degree of which is contemplated, admit the preservation of this property through many grades. It is increased by the addition of the property, or it is increased by a diminution of the medium with which it is in conjunction: it is *diminished* by a diminution of the property, or by an increase of its medium (still in these states preserving its characteristics), or by a causation which may partially affect its nature, or change it past recognising. In addition, therefore, to the dependence of increased degree upon the proportionate quanties of the unmixed property and its medium, a

diminished degree might result from an agency which produces a deviation or change in its identity, and which neither abstracts any portion of the property, nor adds to the quantity of its medium.

If a function were impaired from this latter cause, its condition would be but very indifferently expressed by any demonstration of degree. It implies a change of state, which involves, as a part of its phenomena, a change of degree; but which might have relations both with life and the materials totally independent of degree.

It happens also that a causation which increases the degree of a property, although, according to our analogies, it must always produce this effect by increasing the proportionate quantity of the property, or by diminishing its medium, may, although simple in this respect, be vastly complex in others. If it is the effect of an agent to increase or diminish directly the degree of one property: its relation with others may be of the greatest importance, although the changes or effects which result from this relation may be in identity, and not expressed in degree, because their relation is with no effect which can become the measure of degree. This is the common condition of disease.

To exemplify this state, first by quoting our sensible instances. Four ounces of water, or four ounces of solution of arsenic, or four ounces of a solution of opium, or four ounces of an infusion of digitalis, or four ounces of infusion of bark, or four ounces of an infusion of belladonna, or of

tobacco, or four ounces of a fluid composed of all these, added to four ounces of alcohol, will make a *spirituous* liquor of the same strength or *degree*. Yet although these fluids have with the spirit one common relation, affecting its degree, we should scarcely consider the fluid produced by mixture with the alcohol to be the same in either case, although the degree of the property contemplated may in either case be equal: we should, on the contrary, expect the relations of these fluids, respectively, in connexion with the alcohol, with other substances, to be very different, and consequently their effects, in any processes of causation in which they might happen to be engaged.

It is the same with those changes in animal bodies, some visible signs only of which are effects which result from variety in the degree of ordinary powers. Thus, it may be the effect of hemorrhage to reduce the action of the heart to forty beats in a minute, and in three hours this action may rise to ninety: it may also be the effect of six ounces of the infusion of digitalis to reduce the action of the heart to forty beats in a minute; and in three hours, instead of getting up to ninety, the action of this organ may have entirely ceased. The degree from these different agencies was the same; but although the agents agreed in a relation which was common, and produced a common effect, yet the causation involved the influence of properties which produced a different series of phenomena, of which the degree of the power of action was only one part. Thus also exposure to cold may be followed in one instance

by a rigor and fever, with a pulse of a hundred and twenty, which may subside to eighty in three days; the actuating power of the heart may on another occasion be the same, producing the same rate of action, which may be reduced in twenty-four hours to eighty beats; or it may be a hundred and ten for three days, a hundred for six days, a hundred and ten for three days more; it may reach afterwards a hundred and twenty or thirty; it may then come down to ninety, or it may reach a hundred and sixty; and a course of disease, which commenced with the same degree of actuating power, may be long or short, or terminate in recovery or death, in the respective examples. Thus also in local disease, as of the lungs, the degree of action, so far as this can be judged of, may be the same in several cases: in one there may be only cough, and pain in the chest or side; in another cough, and no pain; in another, cough and expectoration of mucus; in another, expectoration of mucus and blood; in another, expectoration of pus; and these, beginning from the same degree of action, may, exhibiting this diversity of circumstances, manifest also an equal diversity of duration.

It is presumed that no variety of degree will explain the difference between a cancerous wound and one of a simple kind, of the same extent; or that the formation of matter, the effusion of lymph, the aggregation of animal substance as in tubercle, or the deposit of phosphate of lime about joints or in the coats of arteries, can be fairly imputed to the *degree* of any property

which belongs in health to the structures which suffer these changes.

The conclusion from these facts, which are furnished not merely by these examples, but by every instance of disease, is, that the dependence of degree is on a common relation, or one which might be affected by many different causes; that the *state of disease* is identified by causes which produce other effects, with which those of degree are only concomitant; that by these other causes the phenomena of disease which are not to be imputed to degree, are produced; that by these the disposition of disease is formed, and that on these depend principally its circumstances and course.

It has been said, that the doctrine which we are considering, although it is founded upon the variety of the degrees of excitement, proposes no criterion by which this degree may be estimated. We have supplied this omission by regarding the effects of inferred properties as the measure either of their degree or quantity. It has been attempted to show the real importance of the degree with which properties are exerted; and, in our view of this subject, it has appeared, that as phenomena do not admit of explanation on this ground, a more complex causation in disease is to be inferred.

But it appears, that not only is the doctrine of excitement false, both in its physiology, and false in its pathology, supposing even the physiology to be true, but according to our measure of excitement, a treatment founded upon it would be contradictory and absurd. It is concluded from the effects of a property, that its excitement or energy

is too great in fever, because the heart acts with excess of vigour: it must be concluded, by parity of reasoning, that the power of secretion in the kidnies is at the same time in a state of debility, or is preternaturally reduced; because the effect of this power (namely, the secretion of urine) is diminished. It is also to be concluded that a similar debility prevails in the functional power of the liver when the secretion of bile is suspended by inflammation of this viscus.

How then is a fever to be treated by general remedies, such as those which are employed, when, according to the only criterion of power, it is in some organs in a state of preternatural strength, and in others in one of preternatural reduction or of debility? This question, I am aware, may be answered; but only by the aid of suppositions, the postulata of which could scarcely be conceded.

It is also to be observed, in addition to a false physiology, and a false pathology, which this system may boast, that the therapeutics raised on such a basis are doubly untrue: that is, the doctrine of therapeutics is false, because the pathology is false on which it rests; and it is also false in the alleged operation of its means.

The cure of asthenic disease is said to be by the debilitating powers, of which cold is one: yet exposure to cold is generally (perhaps, in an extensive class of disease, always) the cause of sthenic diathesis. The cure of asthenic disease is said to be by stimuli; yet the obvious condition of recovery from fever is a still greater depression of the powers of the constitution, if the effect may be judged of

by the means, after the state of debility may be supposed to have commenced. The chief difference between the cure of fever by nature and by art is, that art reduces the system to the lowest point more rapidly than nature. This point attained, which it is through a range from a sthenic to the extremest asthenic state, the powers of the system will be gradually recovered, even under the continued use of debilitating means, as they are called; and in any stage through this course of debility (so far as such a state may be inferred) an attempt to restore vigour, which only impedes the progress of reduction, protracts the period when the extremest state of debility is arrived at, and from whence the powers of the system will commence the process of recovery. In short, it may happen, that the sthenic diathesis is produced by a debilitating power; and it is very frequently the case, that in diseases of debility the cure is to be accomplished only by debilitating means; as those of depletion. I would be understood that, in this examination of the consistency of the doctrine, the word debility is used according to its popular acceptation; which is that in which it is used in the doctrine to which it relates. Perhaps a precise state is not very clearly suggested by the term, and its use is rather convenient than explanatory in this, as in most other instances.

The principal source from which the doctrine of Brown has derived any practical credit is in the treatment of the constitutional or local effects of cold. In defect of the experience on this subject which we now possess, it would be concluded, very

naturally, that when animation was nearly suspended, from exposure to cold, or when a part was in the state which is called frost-bitten, the proper remedy would be heat. But the information is now popular, that the sudden application of heat to persons, or parts, in this condition, will certainly produce death, where animation was before only almost suspended. The practice, then, of subjecting those who suffer the effects of cold by very slow degrees to the influence of a higher temperature, is one which has reflected some false credit on the doctrine of Brown. The practice has been thus connected with the doctrine. The action of cold is sedative, as they say; the effect of this sedative influence is to diminish excitement, during which there is an accumulation of excitability. If the stimulus of heat is applied to this accumulated excitability, the effect is so violent an increase of excitement as to occasion the extinction of life. This is, doubtless, a very ingenious little specimen of reasoning: a pretty explanation, with which most men may be satisfied.

But the reasoning is liable to these objections: first, it assumes that there is a distinct portion of the principle of life, termed the excitability, which we have found to be not only not proved, but contrary to all analogy; secondly, it assumes that this excitability is capable of accumulating, although it has confessedly no source of reproduction. These objections arise out of our former examinations.

In addition to these, it is to be observed that, supposing the excitability to be accumulated under

the sedative influence of cold, the excitement should not be greater under the influence of the natural stimuli, derived from heat and food, on this account; because the doctrine makes the relation precise, of a certain sum of stimulus with a certain sum of excitability, producing a certain degree of excitement. The effect of cold is to produce that which has been called a state of direct debility; and this same direct debility occurs on other occasions under the natural stimuli of heat, exercise, and a diet, the defect of which is, that it is not sufficiently stimulating to consume the excitability. Now, if these means cannot consume the excitability in one instance, there is no reason why they should in another; if their relation is only with a certain sum of the excitability, producing a certain degree of excitement, there is no reason why a greater degree of excitement should be produced by the same stimuli, whether the excitability has accumulated from cold, or from inadequate means of consumption. If certain stimuli are inadequate to consume the lesser, we should scarcely expect that they would be equal to convert into excitement the greater quantity of excitability—a possibility which our author's explanation, despising trifles, does not hesitate to assume.

Our experience in this case, as in most others, justifies us only in defining relations; we may speak of individual and efficient causes in the department of life, as in other departments, when analysis has made us acquainted with them. At present we can do very little more than state the facts, that exposure to cold of a certain degree, for

a sufficient length of time, will destroy life; that if the subject be rescued from such exposure before life has ceased, and is immediately exposed to a temperature which is considerably higher, the effect will probably be fatal; that the proper method of promoting recovery is by the influence of a higher temperature, beginning with a grade but little above that to which the subject has been exposed, and slowly increasing it.

On this occasion, some may be disposed to say, if the change of temperature be too sudden, the violence of the *reaction* will occasion death—I have no objection to the term *reaction*, if it is only meant to express an effect; it cannot aspire to suggest an explanation. For my own part, I would be content to say, there is a relation between life and cold, by which the former may be made to cease by an exposure to the latter; and that the state of life produced by cold, which is short of its extinction, is so related with heat, that sudden exposure to a certain degree of it will also render life extinct, or have the effect of changing its form: thus, merely stating the fact, that life in one state is disposed for certain effects, and has certain relations; that other relations, and other effects, are the result of another disposition. The phenomena in this case may, indeed, be more precisely noted, and perhaps some circumstances may admit of explanation, by a reference to real causes; but such an explanation must be the result of particular inquiry—the first stage of which must be of a preparatory kind, with a view to establish postulata.

There is one point of practice which has been

connected with the doctrine of Brown, or has perhaps arisen out of it, which is worth retaining; and this is the practice which we have just been considering, extended to other instances; namely, that of a slowly progressive stimulation in those cases of direct debility in which the doctrine assumes the excitability to be accumulated. In these cases we reject the term which affects to designate a state; but the practice of increasing very slowly the means of repletion after considerable privations or depletions, is one, the propriety of which cannot be too forcibly insisted upon.

In the beginning of convalescence from fever, I have known a single meal of a nutritious or stimulating quality, to which the system had been long unaccustomed under the previous treatment of abstinence and depletion, followed by apoplexy, which was fatal in a few hours. Nothing also is more common than violent determinations of blood to the head by sudden repletion after a copious loss of blood, whether from accidental hemorrhage, or by the frequent use of the lancet. I have known such determinations to be frequently produced by this cause in puerperal states; and the effects, of which this determination was the cause or the accompaniment, have exhibited every degree of cerebral disorder, from mere pain and throbbing in the head to delirium, epilepsy, or apoplexy: and I have known the first stimulating meal taken, contrary to direction, twelve days after a delivery which was attended with profuse flooding, followed immediately by accelerated pulse, pain in the head, eventually coma, and death. In

a similar case, a single glass of porter, given injudiciously four days after delivery, has been immediately followed by apoplexy; in which state the woman remained for several hours, and from which she recovered with difficulty. These cases might be greatly multiplied; but this seems superfluous, as the fact does not stand in need of additional examples.

It is common, in these severe and threatening forms of cerebral disorder, notwithstanding previous loss of blood, to resort to the lancet, and to repeat the bleeding, copiously and frequently, if the symptoms continue. So far as I have had opportunities of observation, this practice carried to any great extent, to the neglect perhaps of other measures, has generally had a fatal termination. I have been in the habit of confiding in purgatives, to the almost total neglect of the lancet; but these purgatives have not been of a milk and water kind. If, after such copious loss of blood, the pulse has suddenly got up from eighty to a hundred and thirty or forty in a minute, with intense pain in the head, perhaps slight incoherence of speech, restlessness, &c., I have given six grains of calomel, and the same quantity of James' Powder, followed by a two-ounce draught of salts and senna, with half a dram of jalap in it. If this has been rejected, it has been repeated in less than an hour, and repeated as often as it was rejected, until it has produced copious evacuations from the bowels. One, and perhaps an important, effect of such purgatives, is to make a great revulsion to the whole intestinal canal, which is commonly followed by almost perfect relief of the head, and perhaps an

immediate subsidence of the pulse to a hundred, or a hundred and ten. The tendency to disorder of the head is afterwards easily kept within safe bounds, by small, but repeated doses of Epsom salts; or an exacerbation, if it occurs, may be reduced by a repetition of the former purgatives. If, notwithstanding a full and continued purging, the disorder should persevere in its course, I have reason to think, from the results of cases essentially analogous, that putting the system suddenly under the full influence of mercury is more likely to subvert the disposition to cerebral disorder than the employment of the lancet. But before this measure is resorted to, the fatal termination of the case (unless a vigorous effort is made to avert it) must be so decidedly indicated, that the further prosecution of curative endeavours seems little better than a forlorn hope. In a pretty considerable experience, I have never met with a fatal instance of disorder of the head of this kind when, however threatening and severe the symptoms might be, the plan of treatment by purgatives, nauseating, and perhaps emetic remedies has been wholly or chiefly depended upon. And, so far as my recollection serves me, all the fatal cases which have come to my knowledge have been those in which the treatment of disorder of the head of this kind has been rested wholly or principally upon the use of the lancet.

The alteration occasioned in practice by the doctrines of Brown was generally a mischievous one. Fortunately, his followers are in this country comparatively few. Not but that the practice in-

dicated by his pathology was sometimes the best; but a stimulating treatment is almost always hazardous, and it can be adopted with safety only under the sanction of experience and observation. If it is employed as the dictate of a false principle, it will be right sometimes, but more frequently wrong: and the difference between a practice on a false principle, and one without either rule or observation is, that the errors of the former are systematic; those of the latter, like its successes, accidental.

But, that persons may recover from certain forms of fever under a stimulating treatment, is no proof of the truth of the pathology on which such treatment is founded, unless such a pathology is the only one with which the success of the treatment is compatible. We know that the same diseases may be cured by opposite means, and we know also that these means, though opposite, have an important agreement in this common effect; namely, that they subvert the existing state of disease. It will hereafter be seen, that our best principles of pathology aspire to an end, the character of which is oftentimes not much more precise than this, of changing or superseding the condition of disease. The chief use of personal or individual experience is to supply the place of principles which are unattainable. It is impossible to say from any *a priori* reasoning, in most instances, why one practice should succeed with a certain assemblage of symptoms rather than another: we can only quote relations, and say that, according to our experience, under such circum-

stances, such agents will produce such effects; and such other effects ensue from the employment of other agents.

If, therefore, a case of typhous fever may get well under the use of wine, laudanum, ammonia, camphor, æther, or brandy; and also under the opposite means of bleeding, and purgatives; these means, agreeing in a common effect, do not reflect proof on any pathology which is not equally common to both; and it is the business of experience to decide, where different measures may be proposed for the same end, by which of these measures the end is with the most certainty to be attained.

If the speculative doctrines of Brown have been found liable to insuperable objections, we have not much more reason to be satisfied with the practice which has been partially adopted on their suggestion. This practice has been principally exemplified in the treatment of fever; this consideration has been already touched upon; the merits of the practice will, however, be best appreciated by a brief view of the subject derived directly from our experience.

It rarely happens that fever is unattended with local inflammation of some important viscus. In typhus, the brain perhaps invariably suffers in a greater or lesser degree, either from determination of blood, to quote a sensible circumstance, from inflammation, or congestion. That stimuli of all kinds (more particularly the stimulus of alcohol in any shape) have a tendency to produce determination to the head where it does not already exist, and to increase it where it does, is a fact suf-

ficiently ascertained and confessed. The action of the heart is also quickened by stimuli; and thus a seat of disease may, by their operation, be made to suffer from the additional volume, from the augmented impulse, and from the accelerated renovation of its blood; we know that all these causes increase the symptoms of the disease, and therefore the danger of a fatal termination, to which even singly they may very essentially contribute. I apprehend it is generally, though perhaps not invariably true, that if visceral inflammation or congestion in fever can be kept within safe bounds, very little is to be feared from the constitutional state; and that the disease will almost, without an exception, run a safe, though perhaps a protracted course.

That state of typhus which in this country is called typhus gravior, may be perhaps successfully treated by stimuli: but I believe these are applicable only to its last, or a remote stage. If a case of ordinary typhus is well treated from the beginning, I think we should have very few specimens of this form of the disease.

At a time when, for some two years or more, typhus was exceedingly prevalent, I had opportunities of noticing, in a pretty extensive experience, only the three forms, which have been designated as those of inflammatory, congestive, and atonic typhus. These terms are not unexceptionable; they serve, however, to denote the following examples of the disease, at a period when it prevailed as an epidemic.—As a specimen of the first:

A labouring man, between 40 and 50 years of

age, had severe rigors; these were followed by fever. I saw him eight or ten hours after the commencement of the rigors. He had complained a few hours before of vertigo. At the time of my visit, he had a flushed face, redness of the conjunctiva, delirium, dry tongue, heat of skin, and a full pulse of a hundred and thirty-six in a minute. I took twenty ounces of blood from the arm, gave eight grains of calomel and eight of James' powder, and in an hour afterwards a black draught with half a dram of jalap in it; ordered the hair to be cut close, and the head drenched by clothes dipped in cold water, and frequently renewed. After the operation of the purgative, he took a grain of emetic tartar, every six hours, and in twelve hours from the first purgative, three grains of calomel, and six of James' powder, followed in twelve hours by a black draught with a scruple of jalap in it. The effect of this treatment was to bring his pulse down to a hundred and six in about thirty hours; and if a pulse is so reduced in the first instance, there is no great difficulty in preventing its getting up again. In this man, the disease was brought within safe bounds in about the first twenty-four hours; and if it showed a disposition to exacerbate, I repeated the means, which made a revulsion to the stomach and bowels, but did not again resort to the use of the lancet. By a treatment with a similar design, I have brought a pulse in fever down in twenty-four hours from a hundred and twenty, to fifty-six in a minute, without bleeding. If a disease does not run a safe course afterwards, it will generally be owing to imprudence in the use of stimuli; it may

be protracted, but the disease will spend itself in time; and the constant operation of purgatives throughout its course will serve at once to restrain the action of the heart, to make a derivation from the head, and as a preternatural agency to subvert the condition of disease.

The form of typhus just exemplified would run into typhus gravior, if not severely checked in its commencement; and in some instances, it may run into this form, notwithstanding such treatment; but I have reason, from ample experience, to think that such instances are, most frequently, those in which the inflammatory symptoms are treated in the beginning by inadequate means. To conclude the case; the fever continued about six weeks, but it continued as a subdued disease, and the pulse never after the first twenty-four hours rose higher at any time than a hundred and fifteen or a hundred and twenty: towards the termination of the case, several abscesses or boils formed chiefly about the legs, connected with extensive erysipelatous inflammation of the skin in their neighbourhood. This was a frequent circumstance in the fever which prevailed in Bath, about three years since.

In some instances, erysipelas appeared to substitute typhus, or to show an intimate connexion between these states of disease. Thus, I have known two individuals in a house have typhus in succession, and a third, violent erysipelatous inflammation of one thigh, terminating in abscess near the hip. In an adjoining house, typhus has prevailed; and in the next, there has been a case of

erysipelas of one arm, terminating in abscess about the situation of the elbow. In another house, two individuals have had typhus, and a third erysipelas. In another house, one has had typhus and another a most severe and malignant form of erysipelas of the face and head, with delirium, &c.; in the adjoining house, typhus prevailed, and in a contiguous one, there was a case of the same malignant form of erysipelas. I have also known, in more than one instance, the precursory symptoms, or rather the first stage of typhus, to give place to erysipelas of one arm, and in one of the cases alluded to, typhus recurred a few weeks after the cessation of the erysipelas, and ran a precarious course of three months.

During the prevalence of the same epidemic, a man who was marked from inoculated small pox, which he had when an infant, had natural small pox, which was confluent, and from which he recovered with difficulty: before his health was perfectly restored, he had typhus, lay for three weeks in a state of insensibility, with his tongue and teeth covered with black sordes, and apparently a paralysis of the sphincters of the bladder and rectum, for his urine and stools were perpetually passing from him, as it appeared, involuntarily. This man, whose case exemplified the typhus gravior throughout its course, recovered: I purged him every day from the beginning of the disease to the end, allowed him nothing stronger than gruel or broth, and towards the end of the case employed mercury so freely as to affect his mouth. It is remarkable that the atmosphere of this man's bed-room always

had a most fetid and putrescent odour, from the time of his having small pox to the termination of his typhus, and his breath seemed to be a concentration of putrid effluvia.

The opinion that typhus has two origins, one from external infection, and the other from a spontaneous generation of the disease in the subject affected by it, has been before adverted to, and it appears to me to be confirmed by observation. —Previous to the occurrence of typhus or of erysipelas, or of some other forms of cutaneous disease which also prevailed extensively at the period alluded to, I was always informed, by those who were disposed to remark the circumstance, that the odour of the alvine evacuations was for some time preceding these diseases ammoniacal, or pungently fetid; a state of the contents of the bowels, which it is not unreasonable to suppose might furnish a source of internal infection by the absorption of effluvia of a pernicious quality.

The congestive form of typhus, of which the instances were more rare than of either of the others, may be exemplified in the following case:

A girl about twenty years of age, of a full habit and red face, had rigors, succeeded by prostration of strength, some heat of skin, dry tongue, first vertigo, then stupor, or a sort of lethargy from which she could scarcely be aroused, great dilatation of the pupils, and incoherent speech, when she spoke at all, which was very seldom: her complexion, a deep red or almost of a bluish tint, without augmented temperature of the face, and a small pulse of about eighty in a minute, the

carotids apparently not disproportionally distended. I endeavoured to bleed her, but could not get above three or four ounces of blood from the arm: I gave her six grains of calomel, followed by a black draught with half a dram of jalap in it, and put a blister between her shoulders. Her pulse became fuller after the operation of the purgative, and her head somewhat relieved; the purgative, in rather a less dose, was repeated in twelve or fourteen hours, and the symptoms, after copious and frequent stools had been procured, were so much improved, that her recovery now appeared as certain as her danger was imminent on my first seeing her. The fever afterwards assumed a more regular form, and she recovered from it, in about six weeks, under a treatment by purgatives. This case occurred during the convalescence of the girl's mother, who had had the prevalent form of inflammatory typhus, which also ran a course of about six weeks.

Of the atonic typhus the examples at the period alluded to were very numerous; the symptoms were from the beginning those of debility, as they are called, the pulse rarely exceeding a hundred, or a hundred and ten, and the head but little affected. These cases were generally of very protracted duration, and from the moderation of the symptoms in the beginning, they sometimes proved to be rather insidious. The disease in this form was easily excited to exacerbation, and the symptoms frequently, at the end of three weeks, were more severe than in the first five days. In these cases I tried bark, and weak

stimuli in the form of food; but these means were generally followed by an increase of the symptoms; and, like all the other cases, they appeared to go on safely only under the treatment by purgatives.

It was commonly observable that, in the beginning, and towards the termination of these cases of typhus, in either of its forms, the stools were very dark coloured, or fetid, or both. But the advantages of purging (a conspicuous one of which is that of forcing, by perpetual stimulation, the viscera to relieve themselves by such secretion) were not less obvious in the middle period of the disease. When the first violence of the symptoms was effectually subdued, half an ounce or six drams of Epsom salts every morning, with occasionally a little calomel and James' powder, or a combination of blue pill, aloes, and ipecacuanha at bed-time, with a draught of salts and infusion of senna the next morning, were sufficient to keep the inflammatory disposition in check; and the head was also relieved by a counterdetermination, to use language which is rather convenient than explanatory, to the bowels.

I have reason to think the frequency of the pulse is not diminished in these cases by frequent bleedings: small doses of emetic tartar, or large ones of squills and nitre, I have found, in conjunction with purgatives, more effectual in this design. I have given digitalis with the same view, but it has sometimes produced no effect; and from its operation in larger doses, it has ap-

peared to me an unsafe medicine in the middle, or later stages of typhus. These objections rendered my experience of this remedy in such cases very limited.

In the middle periods of the disease, when the tongue had become tolerably clean, without affection of the head, and the rate of the pulse was below a hundred, I sometimes tried bark, and felt my way, as they say, with stimuli: but it frequently happened, that the patients immediately afterwards complained of pain and throbbing in the head, sense of tightness across the forehead, accompanied with an increase of the pulse of perhaps ten or fifteen beats in a minute. This trifling exacerbation of the symptoms yielded to a purgative, sometimes followed by nauseating doses of emetic tartar. When the increase of symptoms from bark or stimuli was less marked, or threatening, so as to admit a guarded perseverance in this treatment, with the occasional check of a purgative, I have had reason to think the course of the disease was considerably shortened. I found it, however, rarely necessary to resort to tonic measures; the patients gained strength under Epsom salts and gruel, as the disease left them; and I seldom attempted to precipitate their recovery by an opposite plan without losing ground, and thereby protracting their convalescence. Indeed, during the period of convalescence, when patients were well enough to leave their rooms, I have often found that they recovered strength with the greatest regularity while they were kept upon a low diet, and took fre-

quently small doses of Epsom salts. A deviation from this plan, during a tedious recovery, was frequently productive of trifling relapses, which served to protract the disease, although they did not endanger the safety of the patient.

In the treatment of that form of typhus which commenced with violent febrile symptoms, I had seldom occasion to repeat the first bleeding, which was sufficient, in conjunction with other measures, to produce a considerable reduction of their degree. In the progress of the case, at some periods, the loss of six or eight ounces of blood, by cupping, I have thought useful as an auxiliary to a reduced, but regular purging. In severe cases, apparently of the inflammatory form, I have sometimes found the blood not buffed; the crassamentum was, however, in general partially covered with fibrine, which easily yielded to the pressure of a spoon; and even where this appearance of fibrine was visible, the whole crassamentum manifested a weak cohesion. In the congestive form, the blood was rarely at all buffed in the bginning; but it has assumed this state when the symptoms of congestion had yielded to a more marked febrile state. In the atonic form, the coagulation of the blood, as in the other cases, was weak; and this fluid was seldom buffed in the beginning, or at most exhibiting only one small spot, consisting of a thin layer of fibrine: the serum sometimes appeared, even at a first bleeding, disproportionally abundant, and the patients, in this form of the disease, commonly fainted before they had lost above four or five ounces of blood.

On the propriety of blood-letting in typhus, I have reason to think it may be beneficial in the commencement, and the repetition of it topically, or perhaps generally, in small quantities, may be advantageous, if judiciously employed in the course of the disease. I once attended three patients in the same house, who had at the same time the inflammatory form of typhus; their circumstances, their symptoms, and the degree of them, were about the same: the pulse in each was not under a hundred and thirty within the first twenty hours after the rigors, a rate which indicates a dangerous accession of fever. I have reason to doubt whether a patient would recover under any treatment, whose pulse amounted to a hundred and forty in a minute within the first twelve or fourteen hours after the rigor; if the patient does recover, the event will do great credit to the treatment, and the parties interested will also be something indebted to their good fortune. I bled only one of the three patients just alluded to, and trusted to purgatives, &c. with the rest: they all recovered, but the disease ran a more mitigated course in the one that was bled, than in the other two.

A considerable loss of blood, whether by two or three large bleedings, or by many small ones, has appeared to be prejudicial in typhus: the cases in which this practice has been carried to a great extent, are among the fatal ones which have come to my knowledge, and death has been said to have happened from serous effusion into the ventricles of the brain. I would not be under-

stood to insinuate the condemnation of blood-letting in typhus: a practitioner must be free to employ any measures that are indicated, and he should by no means limit his resources, for the sake of consistency, by a systematic opposition to any one remedy.

Of the success of the treatment of typhus chiefly by purgatives, and with the views just expressed, I can speak with confidence. I have myself invariably adopted it, in a pretty considerable experience, and certainly with a success which could be exceeded by no other treatment, for I never lost a patient in this disease, who had not been suffered to go into a state of apoplexy from previous neglect, before I became acquainted with the case. Another exception may be made in the case of a woman, who was attacked with the usual symptoms of typhus ten days after delivery: her pulse was a hundred and forty in a minute in the first twelve hours after the rigors. She was bled twice freely, in order to reduce this great rate of action; the means were however far from producing any such effect: and I have as frequently known the pulse accelerated, as reduced by copious bleeding, both in this and in some other inflammatory diseases. The merits of a treatment of typhus could scarcely be judged by the result of this case, as it is well known that such diseases are more than usually dangerous, when complicated with pregnancy or parturition.

This fever, which justified its appellation of typhus by its being infectious, and by its exemplifying in some instances the form of disease

expressed by the old term putrid fever, was exceedingly prevalent in Bath about four years since. The instances after the summer of 1819 were less frequent; but, from the summer of 1818 to the winter of 1821, the list of patients was never without cases of this description: and the preceding account refers to an aggregate of certainly some hundreds of cases.

Emetics may be useful auxiliaries to purgatives in the treatment of typhus. I have, however, seldom given them except in the form of nauseating doses of emetic tartar or ipecacuanha, which have sometimes produced vomiting. The action of vomiting is less hazardous after the operation of a purgative than before it. I have known an emetic given first, in the commencement of fever, and the patient became apoplectic before the action of it was completely over: he did not survive this occurrence many hours.

When, in spite of the most efficient treatment by bleeding in the beginning, purgatives, &c. typhus has run into the state of coma, and a fatal termination seemed inevitable, and weaker measures have been of little promise, I have found it a successful effort to put the patient rapidly under the influence of mercury, which I have done, by giving ten grains of calomel every six or eight hours. I have known all the alarming symptoms cease as soon as the saliva began to flow freely from the mouth of the insensible patient upon the pillow; and the convalescence has been regular from that period. A boy, fourteen years of age, whose case was a neglected one, who had been ill

of typhus a fortnight without the employment of medicine of any kind, and who had gone progressively from fever, with determination of the head, into a complete state of coma, recovered his sensibility, and understood what was said to him in thirty-six hours, under a salivation, produced in this time, by large doses of calomel and mercurial frictions; he made many efforts to speak, at first unsuccessfully, and it was nearly a week before the power of articulation was restored. This boy recovered so perfectly, as to be no longer under medical care. His appetite afterwards became voracious; and owing most probably to an imprudent indulgence of it, he had apoplexy, which occurred soon after a meal, and died in a few hours.

I have also known coma and insensibility to occur after a three months' course of typhus, beginning with the atonic form, and becoming inflammatory in its progress. The death of this patient (a girl about twenty years of age) was expected every hour. As a last effort I gave her ten grains of calomel every six hours; and although strabismus accompanied the insensibility, in so great a degree that very little more than the conjunctiva of either eye was visible, she began to recover as soon as the saliva ran from her mouth, which occurred on the second day of this treatment; she eventually regained a perfect state of health.

It appears to me, that the state here described, or one nearly approaching to it, is the proper stage for the employment of mercury, with a view to a complete constitutional affection by this re-

medy. At the same time, its employment with this view is not limited to this stage: it is a powerful means of arresting tendencies of disease which threaten to be fatal, if not effectually checked; when, from the character, or degree of the symptoms, the more common remedies appear inadequate to this end. Among other modes of the curative operation of mercury, perhaps the determination to the whole glandular system, of which effect salivation may be considered only as a part, or as the indication of a more general affection of this system, comprising the liver, kidneys, &c. together with the skin, is by no means the least important; and an influence by mercury, short of this effect, would in extreme cases be most probably useless. This general affection of the glandular system is scarcely less positively indicated by the circumstance of salivation than by the stools, which consist commonly, under the mercurial affection, or soon after it, of depraved hepatic secretions of almost every variety. The surface of what might be called counter-determination, is also greatly extended by the action of mercury upon the intestinal canal.

Of the opposite treatment of typhus, as enforced by the doctrines of Brown, by stimuli, and the total neglect of the bowels, I have but little to remark, from my own experience: but some fatal cases which I have heard of were treated in this way, and in a few instances I have had opportunities of witnessing the most melancholy effects from this practice; I have seen patients either stimulated into fatal apoplexy, or suffered

to go quietly into this state. I have seen a case, in which a pulse was allowed to proceed from ninety-eight, without affection of the head, gradually up to a hundred and thirty, with increasing delirium, subsultus tendinum*, eventually coma, apoplexy, and death, without a single purgative during the whole of this exacerbation, and consequently without a stool, during the five or six days which the performance of this tragedy occupied.

This view of the *practice* recommended by Brown, does not appear more favourable than the results of our examination of his pathology. It may perhaps be said, that the system of Brown has been chiefly useful, inasmuch as it has contributed to enlarge our experience, by furnishing us with another variety of treatment; and, by instructing us more fully than we otherwise should have been, in the mode of employing stimulating remedies, and of their effects, an addition is made to our practical resources which may, on some rare occasions, be useful under the guidance of experience and discretion: and this appears to be the little good which has arisen out of much evil.

* On this circumstance of subsultus tendinum I once heard it remarked by that highly respected and intelligent physician, Dr. Falconer, of this city, that he had known many patients in typhus, and many who had subsultus tendinum; but that he never knew one recover after this symptom: my own experience, which has not acquainted me with many instances of this kind, so far as it goes, perfectly coincides with the justness of this remark.

CHAPTER IV.

PATHOLOGY OF THE DETERMINATION OF BLOOD.

It has long been common in medicine, more especially in the explanation of diseases which are obviously local, to speak of determination of blood as their sole or principal cause—very little more has in general been designed by this expression than that parts which are under the circumstances it is intended to denote, contain a preternatnral quantity of blood. But the term in reality, not merely expresses the fact, that a part contains more than its usual quantity of blood; but it hints at the process or mode, by which the part obtains an undue proportion of this fluid: it is not the mere possession of a preternatural quantity of blood, the existence of which in a seat may be perceptible by the senses, but it is *a determination* of blood to this seat, which is a seeming explanation of how it came by it.

This term in medicine is not one of modern date, although its use is perhaps now more frequent than formerly, because the doctrine connected with it is more completely organized. Dr. F. Hoffman was reproached with making the local determination of blood the cause of fever, of inflammation, of spasmodic, and of most other diseases. There appears, therefore, to be no novelty in a doctrine which has been spoken of, and vari-

ously adopted by individuals for more than a century. So much novelty as belongs to the one in question, must be sought for in the additional illustration which experience has furnished, and in the results of collateral investigation, which have been connected with it.

As it has been common to use this term, determination, from the time of Hoffman to the present, so it may be presumed that the great importance of the circulation in diseases has had many advocates. It is, however, superfluous to make any allusion to those by whom this doctrine has been merely entertained: it is indebted for its improvement, perhaps for all the perfection of which it is capable, to the late Dr. Parry, whose works on this subject are written in a spirit of true philosophical research, which has been equalled by few of his contemporaries, and has rarely, in medicine, at any period, been surpassed—yet, by this free tribute to the memory of a man of clear and strong understanding, I would not be understood to acquiesce in his opinions, which appear to me erroneous in some essential respects. But, although those of his opinions which are proposed as general doctrines or principles, appear to be in great measure erroneous, they are nevertheless illustrated by so extensive an experience, and by observation of so much worth, that he who is not acquainted with the results of this experience and observation, will fall short of the most enlightened views of medicine, which are furnished by its present state.

More than thirty years ago, the attention of Dr.

Parry was, I believe, first particularly directed to the importance of the circulation by a case of hysteria, in which, from an obvious determination of blood to the brain, he was induced to try the effect of pressure on the carotids, with a view of diminishing the quantity of blood sent to this viscus. The symptoms were diminished or suspended during the compression of the carotids, and repeated subsequent experiments of the same kind led to the conclusion that the cause of spasmodic diseases, proceeding from disorder of the brain, consisted in a determination of blood to this part.

Thus far there is nothing new in the theory. But, from the obvious connexion of the circulation with cases of this character, the author of the elements of pathology was induced to seek for a similar connexion in other diseases, and as there are few diseases in which there is not more or less of disturbance of the circulation, so it is no wonder that this disturbance or irregularity, being generally remarked, a general importance was assigned to it, equal to that which it had first obtained in some solitary examples; and that which was in the beginning little more than a medical observation, perhaps an accidental one, was at length systematized, and furnished the basis of an universal pathology.

It is asserted by this pathology, that every disease is local, or that it has, at least, a local origin —that the cause of such local disease is that the vessels of its seat contain a preternatural quantity of blood, giving rise, in conjunction with the impulse of the blood derived from the heart, to dif-

ferent degrees of momentum, producing, and corresponding with the varieties of disease. That, as this circumstance is an universal accompaniment of disease, so it is universally the cause of all those phenomena which the seats of disease exhibit. That determination of blood is either simple, or inflammatory. That functions may be disordered by such determination; which, ceasing, merely by the vessels assuming their natural capacities, the disorder of functions occasioned by it may also cease. That the ending of determination of blood is at other times in secretion; and that secretion is the spontaneous mode of relief or cure, when determination of blood, whether simple or inflammatory, occupies a membranous or secreting structure. That mere irritation, disorganization, diminished natural secretions, increase of natural secretions, preternatural depositions of every kind; in short, all diseases, functional and organic, are the children of one common parent, and that determination of blood to the seats of these phenomena, is the cause of them all. The doctrine further states, that a determination of blood to one seat might cease upon the occurrence of a determination to another; and the author calls this the cure of disease by conversion, thereby expressing, in other language, the old observation in medicine, that one disease sometimes cures another.

Such is the outline of a pathology, which is one among the medical systems, which prevail in the present day. The principles merely, if such they might be called, are here stated: the chief value of the author's labours will be found, as before-

said, in the illustration and incidental observations connected with these doctrines, a detail of which does not belong to our present inquiry. Upon the discussion of this pathology, having premised this statement, we may now enter.

The most familiar modes by which an organ or seat may be supposed to obtain a preternatural or disproportionate quantity of blood, may be resolved into three: 1st, By a mechanical obstruction, as to the transmission of arterial, or to the return of venous blood: 2nd, By an accelerated action of the arteries: 3rd, By a preternatural dilatation of the vessels of an organ, or seat, without any mechanical obstruction.

1. Both these obstructions are exemplified by the effects of tumors: thus, if a tumor pressed sufficiently on the abdominal aorta to reduce it two-thirds of its natural area, the inferior branches of this system, being deprived of their usual quantity of blood, and the aggregate quantity not being diminished, there must of course be an increased quantity of blood determined to the parts which are supplied by the superior branches of the aorta, as well as to the heart itself: or, if the vena cava were compressed by a tumor, or the iliac veins, as by a gravid uterus, there would be an accumulation of blood in the parts below these seats of obstruction: or, if either side of the heart, or the great vessels immediately connected with it, were obstructed by thickening, ossified valves, or any other sort of impediment, blood would be accumulated in the lungs, if the obstruction were on the left side; and in the head, and abdominal viscera, if

on the right. These causes of mechanical obstruction are of the palpable kind.

A mechanical obstruction, tantamount in its effect, but of a different sort, has been supposed to be occasioned by a spasmodic state of the vessels. The occurrence of such a vascular spasm has already been considered; and if the evidence cited against it does not amount to a positive refutation, it is at least sufficient to show, that such a cause of impeded circulation is highly problematical.

2. It is pretty generally admitted in physiology, that the arteries have a power of independent action: that they possess the same powers, as are supposed to belong to the heart, of contraction and dilatation, although these powers in the arteries are not exerted in precisely the same way; and that these successive contractions and dilatations in this order of vessels, concur with the heart in the business of the circulation, and are the cause of the arterial pulse. If this kind of action be admitted, it is easy to conceive that a power, which prevails generally in the arterial system, may be increased by any cause of local excitement. The effect of such local excitement would be, according to the theory, to accelerate the successive contractions and dilatations of the arteries in its seat, which would thus obtain a preternatural quantity of blood, proportionate to the increased frequency of pulsation of the arteries in question, compared with that which prevails generally, in the arterial system.

It has been argued, principally of late by Dr.

Parry*, that the arteries have no faculty of pulsation: in proof of which many experiments are cited, which consisted in exposing arteries, and viewing them under every circumstance which appeared calculated to render this action perceptible, if any such action occurred. The result of these observations was, that nothing like contraction and dilatation was to be perceived in a denuded artery: that the only motion was one which was given by the impulse of respiration, or a loco-motion of an artery, at the situation of an angle, from the impulse communicated by the heart.

I have elsewhere professed my belief that arteries not only possess an independent power of contraction and dilatation, but that this action may be locally increased: that this power is auxiliary in the circulation to that of the heart; that it is disproportionate in local inflammation, and is the cause of the increased volume of blood which parts in this state contain.

My belief on these points was decided by the following facts: 1st, That the successive contraction and dilatation of an artery, as that at the wrist, *may be felt:* 2d, That I had found in different examinations, the principal trunk of an inflamed seat beating faster than an artery of a remote system.

The first is a question of physiology, which may, so far as it is connected with our present subject, be discussed with propriety in this place. With respect to the faculty of pulsation, the

* Inquiry on the Arterial Pulse.

proofs on either side may be thus stated*. When an artery is laid bare, for perhaps the space of three inches, or more, it is seen to be a cylinder of a fixed circumference; it is seen to have no alternate movement of contraction and dilatation. When, on the other hand, the artery is examined by the sense of touch, an alternate contraction and dilatation will be felt. Here the two senses of vision and touch are at issue in their testimony.

This apparent contradiction, Dr. Parry attempts to reconcile by observing, that there is no pulse in the natural or uninfluenced state of the artery, which is that when it is merely denuded, but that the pressure upon the vessel, made by an examination by the finger, is the cause of the pulse: that the compression of the artery produces this phenomenon, by diminishing the area of the vessel, which is elsewhere maintained by the volume of its blood, and by the impulse conferred on it by the heart.

But this, it may be remarked, is not precisely the sensation given to the finger in the ordinary way of feeling the pulse; it is not a mere effort at dilatation, by the fluid contained in the vessel; but the artery seems to contract, so that the sense of it is lost, until again its dilatation occurs. This I am now disposed to consider a deception of the

* In a hasty examination, in the shape of a review of Dr. Parry's book on the arteries, in the London Medical and Physical Journal for June, 1816, there is something like a similar exposition of the argument, though it then led to a different conclusion.

sense of touch, which, being impressed, in quick successions, by the impulse of blood from the heart, loses altogether the sensation of a vessel between these efforts at dilatation.

In deciding between these opposing testimonies, it appears desirable that the experiments should be of a kind which cannot alter in any respect the natural condition or relations of the artery. This desideratum can scarcely be said to be attained by either of the modes of examination just mentioned; for in the one case the vessel is deprived of its covering, and in the other it is compressed.

But it has been asked, of what importance can the mere deprivation of integument be, in a case where the examination respects a supposed contraction and dilatation of a vessel from its own powers? Truly of no importance, if the action of the artery is wholly dependent on its own powers: but it is possible that this function may have a relative dependence, which is indicated by the fact, that if, as was remarked by Mr. Hunter, the skin which covered a denuded artery is substituted by any other covering, the movements of contraction and dilatation may be seen as well as felt.

To this it is replied by Dr. Parry, that an artery so restrained, whether by the pressure of skin, paper, cloth, or any other substance, is in that state of compression to which he imputes the pulse. It was suggested in the critique before alluded to, that it is consonant with analogy to suppose that, if muscular fibres were stretched to a certain extent, their contractile power may be

overcome. If such is the fact, the covering of an artery may be essential to its contraction, by restraining the dilatation of it by the blood, within bounds which are compatible with the exertion of the contractile power of its muscular coat.

I have elsewhere* suggested that, even supposing an artery to possess no power of contraction and dilatation in itself, yet, as it is kept in a forced state of distention, it would contract and dilate by the mode of circulation of the heart alone, which impels blood into the arteries in successive quantities. I have argued that, as the volume of blood must be greater in a given space of an artery at one time than at another, so the calibre of the artery must be proportionally increased. To this Dr. Parry has replied†, by objecting to the proof that an artery contains more blood at one time than at another; a supposition which certainly arises very naturally out of the manner in which the heart alternately receives blood, and expels it again into the arteries. Dr. Parry, however, denies that the quantity of blood is on this account greater in a given space of an artery at one time than at another; but he admits "that the velocity is greater:" a concession which will answer the purpose of my argument equally well; for an alternate increase and reduction of the velocity of the blood would occasion alternate states of contraction and dilatation of the vessel which con-

* View of the Relations of the Nervous System.

† On the Arterial Pulse, p. 120.

tained it, and which is "in a forced state of distention," as effectually as if a variable distention were produced by a variable volume of blood.

Not to be tedious in prosecuting an argument which may be discussed on other grounds at some length, it appears, on this question of the alternate contraction and dilatation of the arteries, that the evidence of the senses, which has been appealed to on both sides, is contradictory. Each sensible examination has also the disadvantage of modifying the natural condition, or circumstances of the artery; which is, in general, sufficient to render inconclusive an experiment, the object of which is to ascertain a natural function.

There is a third mode of examination which does not appear liable to this objection; and according to this mode, the uniform distention, advocated so ably by Dr. Parry, seems to be fully proved. If, instead of pressing the finger upon the radial artery, as in the usual way of feeling a pulse, the finger is lightly moved, or rolled transversely over the vessel, its state of uniform distention is plainly felt. This proof, concurring with the evidence of the ocular demonstration afforded by a denuded artery, appears to me so completely conclusive against the alternate contractions and dilatations of arteries, that the reluctance which I formerly felt to giving up a doctrine which at once afforded an easy explanation of some phenomena, and seemed absolutely essential to others, has yielded chiefly to the reiteration of this simple experiment, which I have

made habitually, and almost mechanically, in the course of my practice, upon pulses of every kind, with an uniform result.

The testimony which has been quoted in favour of the contraction and dilatation of arteries, which is derived from the phenomena of inflammation, appears also upon a longer experience, and upon examinations frequently repeated, to be in great measure, if not wholly, fallacious. It is as much as seven or eight years since I first examined the comparative frequency of the pulse of the arteries of an inflamed seat, with the frequency of those at a distance from such seat: and, as I have elsewhere stated*, the pulse in an inflamed seat, as in the foot, finger, or hand, has appeared more frequent than that of an artery belonging to another extremity. This sort of examination has, since the period alluded to, been in some measure habitual to me: and though I have sometimes felt the artery of an inflamed seat beating perhaps at the rate of ninety, and a distant one only at the rate of eighty-four or eighty-six in a minute; yet, in general, I have had reason to think that the rate of pulsation was in both instances precisely the same. It would appear that the result of this examination, which is of the sensible kind, could scarcely admit of any doubt; yet, in reality, it is not so satisfactory or conclusive as it seems to promise. I have not found it possible to count accurately the pulse in two distant seats at the same time: this could easily be done if the stroke

* View of the Relations of the Nervous System.

of the artery were in the two seats precisely synchronous; but this is not the case in health: and it has appeared more irregular under the affection of inflammatory disease. The precedence of stroke in the arteries, appears proportioned to their contiguity to the heart: the pulse of the inguinal artery follows that of the carotid, and that of the inguinal precedes the pulse of the anterior tibial artery on the foot.

The doubt which possibly still attaches to the result of examinations of the comparative frequency of the pulse in the arteries of inflamed and uninflamed seats, is occasioned by the manner in which the examination is made. The only mode of examination, the report of which would be conclusive, is that of both arteries at the same time: but if they do not beat synchronously, it is difficult, perhaps impossible, to count, at the same time, the strokes of each. It therefore happens, that the examinations, with respect to this object, which I formerly made, and those also made by others, are not of the frequency of the pulse in different arteries at the same time, but at successive times: and those who are familiar with disease need not be told that a pulse often varies in quickness four or six beats in a very short space of time: it even happens, not unfrequently, that a pulse may beat twenty strokes in the first quarter, twenty-two in the second, nineteen in the third, and perhaps twenty-one in the fourth quarter of a minute; and that too without being very strikingly intermittent.

As the comparative frequency of the pulse in

inflammation cannot oftentimes, or generally, be pronounced upon by an examination of remote arteries at the same time; and as an examination of one, and then the other, in succession, is not a criterion of their comparative frequency, by reason that the action of the heart itself might have undergone a change between the two examinations, it has occurred to me to examine the radial artery first, and then the anterior tibial, when the foot has been violently inflamed, and then the radial again, and to interchange the examinations in this order, with the following result: the pulse, at the wrist, has been eighty-four; of the anterior tibial, in the next minute, eighty-six; the radial, in the next, eighty-four; and the anterior tibial artery, in the next minute, eighty-two. From having repeated this kind of experiment many times, I am inclined to think the result of a general examination of the fact in question, of the same kind, would be a decision in favour of the equal pulsations of the arteries of inflamed and of uninflamed seats: in other words, that both correspond with the action of the heart; and consequently, that a proof of an independent faculty of contraction and dilatation belonging to the arteries is not to be derived from this source *.

Although I incline to the opinion that the proofs

* This successive examination of the pulse in different arteries has been objected to by Dr. Parry (Inquiry on the Arterial Pulse, p. 137), who suggested that two persons should feel the arteries at the same time. I have found, under an examination by this mode, a correspondence in the frequency of the pulse in the arteries of an inflamed, and of a distant seat; but as this experiment has been made only in a solitary instance, I cannot speak of the uniformity of this result.

are stronger against, than in favour of the alternate dilatation and contraction of arteries, yet it must be allowed, in the first place, that all these proofs are not uniform in their occurrence; and in the second, that there are circumstances with which the supposed absence of this faculty is at least in analogical disagreement: such as the facts, 1st, That an artery will empty itself of its blood beyond a ligature; 2d, That there is more blood, or, as is conceded, a greater velocity of blood, which is much the same thing, in a given space of an artery at one time than at another, which effect would ensue merely from the blood being thrown into the arteries by the heart, in successive quantities; 3d, The irregular manner in which blood is projected from a wounded artery; 4th, That no visible pulsation of a denuded artery is produced by a ligature, whether the vessel is partially or completely constricted by it, which one would expect to be the case, if the pulse were given, as explained by Dr. Parry, by a circulation locally impeded, and a consequent effort on the part of the blood to restore the calibre of the artery in the situation of such impediment. To these circumstances must be added the irregularities or disagreement of the pulse of different arteries which are certainly sometimes observable, if the mode of examination is in any degree to be trusted, under inflammatory disease. These facts collectively will leave still, I fear, some shade of doubt on a point of physiology which is both interesting and important. It furnishes no boast of modern researches in physiology, that, although the circulation of the blood has been discovered nearly two

hundred years, the manner in which this circulation is accomplished is not only not ascertained, but, as a matter of opinion, is one on which physiologists are not agreed.

The theory of the alternate contraction and dilatation of the arteries, has derived considerable support from the supposed inadequacy of the heart to perform the circulation, without some such assistance on the part of the arteries. To imagine that the heart alone is capable of impelling a weight of blood, amounting to many pounds*, by a vis a tergo, through the immense range of the vascular system, consisting of arteries, secerning structures, and veins, under every variety of position; to which also is perhaps to be added, the transmission of fluids through the lymphatics, would indeed be to impute a force to this organ approaching to that assigned by Borelli, which was equal to a weight of some thousands of pounds; and a force of injection necessary to accomplish a circulation of such extent would assuredly burst any vessel upon which it might be exerted. But the only mode in which the force of the contraction of the heart can be calculated, is according to that which it may be found absolutely to exert: and this may be judged of by the projection of blood from a wound of the aorta, contiguous to the heart. The distance to which blood would be thrown would not exceed a few feet, although, as must be taken into the account, the heart would not in this

* It was conjectured by Haller that the whole mass of circulating fluids amounts to fifty pounds, of which, one fifth is red blood.

case have to propel the whole mass of blood, but only as much as was contained in the left ventricle, and the space intervening between the wound of the aorta and the heart. As we know that it requires very little muscular exertion, by means of a syringe, to project a fluid not only a few feet, but some yards, so we must conclude that the actual force of the heart's contraction, instead of being equal to a weight of three thousand, comes much nearer to that of eight pounds, which was the computation of Reil.

Thus, the inadequacy of the heart alone to perform the circulation, appears too obvious not to be readily confessed; and if it is proved, as upon the whole I think it is, that the arteries have no mechanical action by which they can contribute towards the circulation, this phenomenon, in the present stage of our physiological progress, remains absolutely unexplained.

As the mechanical powers appear insufficient to account for the circulation of the blood, it is necessary that we should look for the explanation of this circumstance to some other recognized agents in the animal economy. Our analyses teach us that there are but three sets of properties discoverable in animals: and, that these are either mechanical, chemical, or vital; that all the phenomena of animals are produced by these separately, or in the way of co-operation. In this business of the circulation, it appears that the mechanical powers do not afford an adequate cause; and the process is so little analogous to chemical agency, that it is not worth while to inquire, whether properties belonging to this class may be in any de-

gree engaged in it. The only alternative which remains, according to this statement of the argument, is, that so much as the mechanical power of the heart wants, to perform the circulation of the blood, is in some way supplied by the influence of that class of properties which has been denominated vital. Little more is at present confessed with respect to the arteries than that they are passive tubes, endowed merely with a vital, or tonic, and an elastic power of contraction.

But it is probable that the different orders of vessels have powers which are relative to the fluids they circulate, or to those exposed to their action, which are additional to those of mere contractility. That their vital properties are capable of elaborating changes on their fluid contents, and of producing many phenomena which occur in the course of the circulation, and which certainly cannot be explained in agreement with any mechanical laws.

Something equivalent to the agency alluded to is tacitly and perhaps vaguely confessed with respect to the secerning extremities of arteries, whether in the common or parenchymatous structures. It is pretty generally admitted that the properties of life are actively engaged in producing the secretions; which is an instance of the exertion of properties of the vascular system upon fluids, which is independent of, or at least superadded to mechanical agency.

We see also the mesenteric absorbents exercising a faculty of separation, by which they obtain uniformly a similar fluid, from amidst the heterogeneous mass with which men, to gra-

tify their palates, take the liberty of filling their bowels. Not only do these vessels separate an uniform fluid proper for nutrition from this mass, rejecting all others, even the colouring matter of bile, but they absorb this fluid, and convey it into the sanguiferous system; a process which can in no stage of it be explained without a recurrence to the agency of these same mysterious properties of life.

If, then, it is acknowledged necessary that other properties of life should co-operate with the action of the heart for the purposes of the circulation, it is next to be inquired what these other properties are? and if this inquiry should fail, to seek afterwards for some analogical explanation, which shall be in some way connected with the agency of these properties.

It need scarcely be suggested, that an inquiry in the first place, which respects the identical nature of the properties in question, is not likely to be in any degree successful. Our present business is not with a demonstration of the nature of these properties, but with respect to the mode in which, assuming their agency, they might contribute to the effect which, without their aid, seems to be but imperfectly explained.

The alternatives appear to be either, 1st, That the vital properties of the vessels, by an immediate influence upon the blood contained in them, assist in the transmission of this fluid throughout their respective systems: or, 2d, That this end is promoted by a function of the capillaries.

1. That other properties of arteries have a re-

lation with the blood they contain, besides those of mere contractility, is, I think, proved by the circumstance, that the fluidity of the blood is most probably owing to some such additional faculty. It is common to consider the fluidity of the blood as the effect of its motion and temperature alone; but if an artery is wounded an inch below the surface, it will at first bleed profusely; the stream of blood gradually diminishes, and in the end spontaneously stops. The process of this spontaneous cessation of hemorrhage is, that the artery, if divided, retracts within its sheath, the blood which is injected into the cellular membrane coagulates, the blood also coagulates within the sheath, and finally within the artery itself. In this process there is a progressive coagulation of blood external to the artery; but if mere motion and temperature were the only causes of the fluidity of the blood, there is no reason why it should coagulate in the theca of the artery, or why the channel through which it first flowed freely, through some depth of cellular membrane, should not be preserved; constituting a canal, the *sides* of which only ought to be filled with the coagulum, which, being lodged in membranous cavities, was no longer subjected to the impetus of the circulation. If the mere motion and temperature of the blood preserved its fluidity, this motion and temperature are the same, whether the canal through which the blood is projected is membrane or coagulum; or whether its sides are formed by the intermixture of these substances, or consist of an arterial tube.

It may be said that, although the motion of the blood may be the same in the sheath of an artery so circumstanced, as in the vessel itself, yet the temperature is not the same; that the blood first coagulates which is exposed to the external air, and this impediment becoming complete, the coagulation is continued, in this direction, to the mouth of the vessel. But that this is the fact is by no means certain: it has happened to me, and no doubt to others, that after opening the temporal artery, the stream of blood, which was at first considerable, has gradually diminished, and finally ceased, although the impediment of a coagulum in the situation of the wound, which was exposed to the atmosphere, has been prevented by frequent sponging with hot water, by separating the edges with a probe, &c. thus proving, that if the coagulation does not commence at a point nearest the mouth of the vessel, the absence of an external impediment is not sufficient to prevent this coagulation.

We observe also, in aneurism, or in varix, in which condition it may be presumed the vessel loses locally the power of resisting the disposition in the blood to coagulate, that layer after layer of coagulum forms, until the calibre of the vessel, in this place, is less than it is elsewhere; which does not appear likely to happen, if the fluidity of the blood is preserved merely by motion and temperature. Further, it is remarked by Dr. Parry*, that the blood would not pass through a quill, which

* Inquiry on the Arterial Pulse, p. 44.

was secured within a carotid artery, in consequence of its coagulation.

It is an obvious answer to this supposed power of the blood vessels to preserve the fluidity of the blood, that the blood coagulates in the arteries themselves, both after wounds, and after the application of a ligature. This circumstance proves only that this power in arteries may be impaired or destroyed, alike by external injuries and by disease: mechanical impediment to the circulation may be sufficient to prevent the exertion of this power, which, being antagonist to the disposition of blood to coagulate, may be equal to counteract this disposition only so long as the motion also of the blood shall concur to this end.

But if it be admitted that arteries possess the power of resisting the coagulation of the blood, this power alone cannot be supposed to assist in the circulation, any further than by preserving a state of the blood which is favourable or essential to this purpose. If the arteries do not contribute towards the circulation in the mechanical way of contraction and dilatation, we can scarcely conceive a possible mode by which their vital properties may be directly concerned in promoting this end. Unless, indeed, it were imagined that the nervous influence communicated to arteries was perpetually propagated in the direction from their origin to their extremities; conveying, by affinity with the blood they contain, this fluid through the same course; and either endowing it with the impetus, which furnishes an adequate vis a tergo, for effecting its return to the heart, through

the veins, or conducting it through these vessels by a similar mode—a conjecture which is so wholly unsupported, and so very wide of the mark, that it may, without much hesitation, be dismissed.

2. Our second alternative, namely, that an auxiliary power to the heart, in the business of the circulation, is found in the capillary system, is perhaps one of a less visionary character.

One of the terminations of every artery—of every minute branch of an artery, is in secerning extremities; and so extensive is this secerning system, that there is perhaps scarcely a definable space in the whole organization of an animal into the composition of which this system does not enter. The importance of the objects of this order of structure is fully commensurate with its prevalence and extent. It is by this structure that all the recognized secretions are furnished; it is by a function connected with this structure, as may be demonstrated, that arterial is converted into venous blood; and it is by the products of this structure, which pervade every part of the fabric, that the materials of growth, of nutrition, and of life, are distributed.

For purposes so important, it is naturally to be expected that the provisions for the function of this system are of no ordinary kind: and that they should be so determinate, and of so predominating a force, as to leave nothing doubtful in the accomplishment of their end. We see this secerning system uninfluenced by hydraulic laws; nay, superseding and setting them wholly aside, securing its own objects by these agents of life, which, in

the economy of animals, have always the first command. By the agency of these properties we see the vessels of this system making their peculiar separations; we find the minutest order of tubes separating the thickest fluids, and rejecting the thinnest; we see those of a larger order conveying thinner fluids, and rejecting those more viscid ones, which abound in the common mass from whence the selection is made. We witness, in short, in this system of vessels, the perpetual exertion of an influence which entertains a specific affinity for those fluids which it is their function to separate from a common material. It is to the exertion of this affinity in the capillary system, that I am disposed to look for an auxiliary power to the heart, in the performance of the circulation *.

This explanation does not suppose the extension of a power of affinity to considerable distances from its seat; the power resides in the terminations of vessels (or in the structures which they pervade), which are but a few inches from their trunks: its influence, therefore, in conveying the blood through the arteries, does not proceed from any distant source, but is exerted in every stage of the progress of blood through these vessels.

If it were demonstrable that a power of attraction of any other sort existed at the extremities of all the arteries, should we be at any loss to explain

* It has been remarked by Haller, that the circulation is continued in the small arteries after the application of a ligature to the aorta, or even after the heart has been removed; a circumstance which agrees perfectly with the theory here suggested, although it has not, to my knowledge, been hitherto explained.

how blood acquired its impetus? or how it was conveyed through a course, at the terminations of which, such an attracting power resided? We should without doubt consider this provision so completely adequate, that we should scarcely imagine any other necessary; which indeed is proved to be the case by the fact, (or is at least in agreement with it) that the circulation, in some animals, is maintained without a heart; in others, this organ is so imperfect, as to be of little use; and in the crustaceous ova, it is remarked that the circulation has commenced before the formation of the heart is completed.

The sufficiency of such a power of attraction at the extremities of arteries, for all the purposes imputed to it, would be readily conceded, provided this power were only of a recognized sort. But, if the existence of the power be real, as it is presumed is fully proved by its inferred necessity in the work of secretion, it matters very little how it is denominated, whether one of capillary attraction, or of vital affinity; the latter, however, will be adopted in consideration that the phenomena dependent upon it are peculiar to the living state.

It may be owing to this power in the extremities of arteries, that their trunks or branches are emptied on that side of a ligature which is furthest from the heart, which occurs even if the position should be such, that this power is not only unassisted, but actually opposed by the gravity of the blood. As universal death is preceded by that of the organs which maintain life only by their relation with blood; as those of respiration, the brain,

and perhaps the heart; and as the principal end of the function of the capillaries is the support of the life which belongs to every part of the fabric, so it may be presumed that the secerning function outlives the effective powers of the organs first mentioned; and hence, rather than from a faculty of tonic contraction of the arteries, which in the trunks or principal branches does not go the extent of obliterating their tubes, the arteries are found invariably empty after death. It is not improbable but that the evacuation of blood from the arteries, at the time of death, may also be, in some measure, connected with the state of the lungs. The action of the heart survives the function of respiration, the cessation of which is followed by a collapse of the lungs. This collapsed condition of the lungs, as may be proved by much collateral testimony, is sufficient to impede the transmission of blood from the right side of the heart, through the pulmonary artery, in the enfeebled state of the powers of circulation which immediately precedes death. Hence, the circulation of the blood stops in the venous system. The impetus with which the blood is endowed during life, may furnish a fourth alternative of the cause of the phenomenon in question. —This impetus, a reduced one, may be sufficient to carry the blood through the arteries, but unequal to return it again through the heart, into the arterial system. Either of these causes, except the alleged contraction of the arteries, seems adequate to the explanation of the effect; whether they concur in this effect, or whether their agency

is exclusive, may most probably be ascertained, if the object were of sufficient importance.

Against this co-operation with the heart on the part of the secerning extremities of arteries, it will be objected, if blood is conveyed to these vessels by a power of affinity or attraction, the circulation ought to stop here: there is no reason why blood should proceed further than the seat of the power towards which it is impelled. But in reply to this objection it must be remembered, that the power of secretion is not related with the blood, but with certain parts of it, which are so mixed, as to make apparently one homogeneous fluid, until it is subjected to the analysis of the secerning function; until that portion is separated, in every seat, which the secerning structures respectively affect. The relation of the secerning power is with certain constituents of blood: the secretion made, the affinity with the blood ceases, and its power is repeated on fresh quantities, leaving that blood from which the separation has been made endowed with an impetus which concurs with the vis à tergo of the additional quantities of arterial blood brought to the secerning system, and probably also with a power of attraction in the capillaries of the venous and absorbent systems, to accomplish its return to the heart.

Chiefly from the premises already stated, as well as from illustration which will be subsequently added, it appears to be little less than demonstrated, 1st, that a power of attraction in the extreme distribution of the arteries is necessary to co-operate

with the power of the heart, in the circulation: 2nd, that this being a power of derivation, the blood, in seats, is in a ratio to the capacity of the vessels, and to the degree principally with which this power is exerted, with which also, varieties in the force and frequency of the heart's contraction may concur: 3rd, that this power is one of life, or does not belong to inanimate matter; and, 4th, that it is connected with another function, namely that of secretion, which subserves also other purposes in the animal economy, which are recognised in the physiology of the secreting systems *.

A mode of substituting experiment for inference on this point has occurred to me. The first point to be proved is that the blood in the arterial trunks is derived from them by a power of attraction, in the distribution of their branches; the second, that this power is not one of the inanimate fabric.

It is a well known fact, that an artery, as the aorta for example, becomes empty below a ligature; but this might arise, as before-mentioned, from two other causes, than the one I have supposed: the first is, that the blood may be expelled by the tonic contraction of the artery; the second, that the impetus which the blood possesses may carry it through this portion of the vessel. In an experiment, with the view just suggested, both these

* The power, the intervention of which for additional purposes is here supposed, I suggested some three years since, connecting it at that time only with the function of secretion (see Indications on Organic Life, p. 247. § 22, § 23. p. 248. § 26, &c.

modes must be prevented; and this may be done, by substituting three inches of a silver or elastic gum tube of a proper temperature, for three inches of the artery: the blood is to be allowed to flow through this extent of an inanimate tube; a ligature is then to be *strained* on that portion of the artery beyond the tube, which is furthest from the heart; a ligature is to be *tied* on the portion of the artery nearest the heart. By resting a second or two in the tube, the impetus of the blood in it would have ceased; the ligature furthest from the heart, which is only strained, is then to be loosened, and the position of the part to which the artery is distributed made perpendicular, or such, that the gravity of the blood may not assist its passage from the tube: the ligature furthest from the heart, that which was only strained, is to be tied, after three or four seconds have elapsed; and it is then to be examined, whether the blood contained in the tube has escaped from it: if it has, against its own gravity, and without the aid of an impetus, and under circumstances which precluded its evacuation by a contraction of the tube which contained it, it appears a legitimate inference that some power of attraction in the distribution of the branches assists in conveying or deriving blood from the trunks.

In order to ascertain whether this power is one dependent upon life, the experiment may be repeated with this difference; that the tube at the end most remote from the heart may be connected to an artery in a dead subject, the animal

temperature of which might be preserved by immersion in warm water. It would then appear by the examination of the contents of the tube, whether the evacuation of blood in the former instance was to be imputed to a power of the living state, or whether it was owing to a property, which belongs equally to the vascular system of inanimate structures.

In order that the above experiments should be satisfactory, it is necessary that they should be performed on the arteries of some large animal, as of the horse, the want of an opportunity of doing which, together with a general aversion to experiments on living animals, have prevented my appealing to this test. From my own knowledge, however, I can assert, that if a portion of blood is inclosed in two inches of the aorta of a rabbit, and allowed to remain in this portion two or three seconds, the supply being cut off by a ligature on the side nearest the heart; on withdrawing a strained ligature, on the side furthest from the heart, the blood will be evacuated from this portion of the artery, even though the position of the animal should preclude the assistance of the gravity of the blood, or although the gravity of the blood should be made to oppose its escape. This portion of the artery will appear loose and collapsed, rather as if from the removal of a cause by which an elastic tube was distended, than as if from the exertion of a tonic power, which would, in some measure, preserve the circular shape of the vessel, and give to it a certain degree of rigidity. Some difficulty has been experienced in confining blood

in an artery between two ligatures. This was done, Dr. Parry remarks, "at last, by straining two ligatures at the same instant." Blood might readily be confined in any portion of an artery which does not give off one or more branches, by straining *first* the ligature which is furthest from the heart.

Notwithstanding the certainty which the above experiments appear to promise, I fear their results would be liable to some speculative doubts derived from considerations of analogy, or from ascertained laws in some other branches of physics. I had rather, therefore, trust the opinion I have hazarded on this subject to the support of general experience, and to that of its conformity with all that is inferred to be necessary, and all that is actually witnessed, in the phenomena of health or disease. Indeed, however this view of the subject may be denominated, whether an hypothesis, a theory, a conjecture, or a dream, I have no hesitation in saying, that it is explanatory of more phenomena than any other which has been proposed: and this, I trust, will appear from a more detailed consideration of these phenomena.

The circulation through the veins has been attributed (1.) exclusively to the systoles of the heart; (2.) to the co-operation of the alternate contractions and dilatations of the arteries with the heart; (3.) to a capillary attraction of the blood by the venous origins (supposing that arterial terminations and venous origins are not continuous, but separated by an intermediate, interstitial structure); (4.) to a power of suction,

caused by the dilatation of the right auricle, by which a vacuum is produced; (5.) to the action of the muscles, pressing forward mechanically the blood in contiguous veins; (6.) to the co-operation of the action of the heart, with a power of derivation in the arterial capillaries, or in the structures to which they are distributed. All these modes, except (4.) and (5.), are supposed to accomplish the return of blood through the veins by a vis à tergo. Of these powers of venous circulation, the action of the heart during its systole, the impulse which the blood receives by a power of derivation in the arterial capillaries, the power of suction produced by the dilatation of the right auricle, together with some aid from the action of the muscles, make a co-operation by which the circulation through the veins may be best explained, in agreement with sensible, or analogical testimony.

To these modes might be added conjecturally, that the blood suffers a decomposition, or complete separation of its constituents, by the function of the arterial capillary terminations, or by that power of affinity in them before mentioned; that these constituents of blood, having served the purposes of nutrition, are absorbed by a similar vital power of affinity, which becomes another origin of a power of circulation, on the one hand, through the lymphatic, and on the other, through the venous, systems; that the constituents of arterial blood, collected by absorbents terminating in the veins, are in these vessels reunited, forming venous blood. If this theory were connected with

the function of respiration (by which it is chiefly suggested), it would suppose that the oxygen of arterial blood, in the seats of secerned fluids, supports life, the consumption of which is unremitting; that the fluids which form venous blood obtain carbonic acid (thus appropriating, in another combination, the oxygen which before supported life) by the conversion of the living principle into that informal state which constitutes its death; that the venous blood is decarbonized during expiration, and arterial blood produced by the absorption of oxygen during inspiration; which relations would preserve to oxygen the character of a supporter of life, and would render equal the quantity of oxygen imbibed in the lungs, consumed by supporting life in the structures, and expired in the form of carbonic acid. The source of carbon, in venous blood, will be looked for in the material supplied by digestion, &c. If the proposal of so crude a conjecture (which might easily be reduced to experiment) stands in need of apology, it may perhaps be excused on the ground that the truths which are confirmed by some individuals are frequently suggested by the errors of those who preceded them.

It has been supposed that the lymphatics, like the arteries, propel their contents by an alternate contraction and dilatation. But if this power is not discoverable in the arteries, it will, I apprehend, be sought for in the lymphatics with slighter probability of success. At the same time, if the power which we have assumed to belong to the

secerning structure be conceded, an inquiry after a faculty of pulsation in the lymphatics seems superfluous; for the unremitting exertion of the secerning function will provide a vis à tergo, which will promote the transmission of fluids through the absorbents; an order of vessels which, I believe, are supplied by extravasated or interstitial fluids, poured out from the secerning systems, and for the purposes of life and nutrition, pervading and renewing the minutest particles of which the structures are composed. If this vis à tergo should be inadequate, an additional power, similar to that of the arterial, may be conjectured to reside in the returning capillaries of the circulation: of this conjecture with our present design it is necessary to say nothing further in this place, than that such an origin of a power of circulation in the absorbent orifices possesses in its favour a testimony of analogy, derived from the function of the absorbents of the mesentery.

If the supposed agency of a power of attraction in the extreme distribution of the arteries is found to agree with all the circumstances of physiology with which it might be connected, or even to be explanatory of some for which it was not expressly cited; it will, I trust, appear equally compatible with the circumstances of disease, or possibly, as we may hereafter find, explanatory also of some of its phenomena. To call it a theory is, I am aware, with many persons, to sign its condemnation with my own hand. I can as correctly appreciate the degree of dependence which is to be placed on theory as those who express

towards it the greatest hostility: but, for want of better information, we must nevertheless trust to it in nine actions of our lives out of ten; or perhaps in a much larger proportion. But if an entire confidence is rarely, if ever, to be reposed on matters of opinion, I think nevertheless that new inferences from ascertained facts, which will serve to direct demonstrative inquiry to precise and important ends, may be quite as useful as the immethodical multiplication of experiments, which may boast only in general of having inflicted much unnecessary torture, and of having added, by the increase of inconclusive facts, to the chaos of data which we already possess.

To return from this physiological examination to the subject of the determination of blood. We have discussed two alternatives of the mode in which this has been said to take place: 1st, by a mechanical obstruction; and, 2nd, by an accelerated contraction and dilatation of the arteries. The instances of the first have been alluded to: of the second, the possibility of this mode has been rejected, on the grounds that no such contraction and dilatation is perceptible in arteries when denuded; and that an uniform distention of an artery may be felt when the circumstances of the vessel are perfectly natural, by a modification of the ordinary manner of feeling the pulse. The third mode of determination, namely, by a dilatation of the arteries of its seat, remains to be considered.

3. The pathology in this instance, as in all others, should, if possible, be founded on the physiology of the system to which it relates. In

order, therefore, to ascertain by what mode the vessels of an inflamed seat acquire a preternatural calibre, it is proper first to inquire what those powers are by which the calibre of vessels is determined in the state of health.

The arteries are found to possess two powers of contraction; an elastic one, which operates after complete death, and a vital or tonic power of contraction. Both these are, in general, overcome during life by the momentum of the blood. The proofs of these powers are, that the circumference of an artery is greater during the living than the dead state; and that its circumference is less immediately after it is deprived of the distending force of the blood, before, as is presumed, its life is extinct, than it is when life has ceased, and when its calibre is determined only by its elasticity. The powers of dilatation and of vital contraction no longer operating, the vessel possesses a mean area, between that to which it would be enlarged by the momentum of the blood, and that to which it would be reduced by the exertion of its tonic power *. This is all that is actually discovered with respect to the powers of arteries, which relate to their calibre.

It has been conjectured, in addition to these two powers, both of which concur to resist the distention of the arteries by blood, that these vessels have also a power of dilatation. The inference of this power appears to have arisen, in great measure, out of the theory which has prevailed

* This subject is well illustrated in Dr. Parry's Inquiry on the Arterial Pulse.

with respect to the cause of the pulse; namely, that it is occasioned by an alternate exertion of the two faculties of contraction and dilatation. But as this action of the arteries has been rejected on grounds which it is superfluous to repeat, so the existence of a power of dilatation, which has been inferred only agreeably with this supposed action, cannot, for the same reason, be admitted.

It has been quoted that the heart possesses this power of dilatation, which manifests itself in an animal bled to death, in the continued dilatations and contractions of its ventricles. The contraction is, in this case, sufficiently obvious; but whether the dilatation is owing to a vital action, or whether it is the effect of elasticity, exerted when the contraction has ceased, is not satisfactorily decided. Whether or not these two powers are possessed by the heart is, in some measure, a separate question; that they are exercised by the arteries is contrary to the evidence of the senses: the mere indications of analogy must therefore give place to this superior proof.

A general power of dilatation of some sort in the vascular system has, I believe, been commonly admitted, if not frequently defined, by pathologists. The enlargement of vessels in all states of inflammation, and in the other instances of determination of blood, which comprise almost all diseases, is so conspicuous, that a dilatability of the vascular system is no speculative point, but is a fact which the most careless observers must have remarked every day. In some former publications I have professed my belief that there exist in the

vascular system two powers, one of contraction, and the other of dilatation*;—that the action of the latter is the immediate cause of the determination of blood. Thus, in considering the occurrence of inflammation (comprising a dilatation of the arteries) from external injury, for example; I have remarked that the cause of such injury must be related with a property of life, since it is not productive of any circumstances of inflammation in the dead subject: that if only two powers are assigned to arteries, an elastic one, which belongs to inanimate matter, and a tonic one, which is one of contraction, there is no power by which the enlargement of vessels under irritation or disease can take place. That such dilatation does occur, as before remarked, is part of our experience; and if this dilatation of the vessels "happens neither by their tonic power, nor by their elastic power, a third must be inferred, which, in agreement with perceptible phenomena, may be called one of dilatation †."

But that a power of dilatation is possessed, by which the area of the vessels of a seat might be increased, I have said is not demonstrated by any experiments which have been made on this system: on the contrary, experimental testimony is rather against this supposition. Although the inference

* View of the Relations of the Nervous System, pp. 97, 101, 102, 103, &c.

† See a hasty Review of Dr. Parry's Inquiry on the Arterial Pulse, in the London Medical and Physical Journal for June, 1816, p. 482.

of this power once appeared to me to be legitimately deduced, it was chiefly because there appeared to be no other alternative, rather than that the explanation it offered was not open to objections. If, therefore, a less exceptionable explanation is suggested, the force of these objections might be sufficient to invalidate the conclusion.

If a power of dilatation in the arteries is not demonstrable, it can only be inferred; and the inference can be made only from analogy. The chief objections hinted at are those furnished by analogy. The only means by which an artery, for example, can possess the opposite faculties of contraction and dilatation, must be those of a double set of muscular fibres; or else one set must, with peculiar arrangements, which would render the circumstance barely possible, possess the power of inverting the order of their contraction. The effect of a contraction of one set of fibres may be a diminution, and of the other an enlargement of the area of a vessel.

But we can scarcely imagine that a stimulus, acting on the same structure, should have the effect of exciting one set of fibres, and not the other, when both must be so intimately mixed, that if they exist, their distinction is not to be discerned by the minutest scrutiny. If this structure is not apparent, we have no right to infer it, except on the ground of analogy to some unequivocal examples. The whole animal economy furnishes, I believe, no example of the *same muscular tissue*, with no perceptible diversity of fibrous

arrangement, possessing antagonist powers of contraction.

It is probable that some might be disposed to suggest the iris as an instance of the possession of this double faculty. It would scarcely be worth while to remark on this point, but that this structure has been quoted in the consideration of our present topic, though the testimony it affords has been allowed no great weight*. But on this example, it is to be observed, that the tissue of supposed muscularity is not the same; that if the pupil is contracted by one muscular exertion, and dilated by another, there are in its structure, at least, the appearances of distinct muscular arrangements; of circular, and of radiated fibres, corresponding with these purposes.

But I cannot admit that the pupil is dilated by a muscular contraction of any sort, until the proofs adduced are stronger than any which have hitherto been proposed. It appears that the dilatation of the pupil is rather owing to the absence of muscular contraction than to any exertion of contractile power. This seems probable, when we consider that it is the presence of a stimulus which causes the pupil to contract, and the absence of stimulus which occasions the pupil to dilate. Thus it contracts on exposure to the light, and dilates upon the cessation of this stimulus, or in darkness. It is also the effect of a narcotic poison, of belladonna, a paralysing power, to produce a dilatation of the pupil; and in paralysis, occasioned by pressure on

* Parry's Elements of Pathology, p. 25.

the brain, as in apoplexy, there is dilatation of the pupil: which facts appear sufficient proofs that the pupil dilates from the reduction of a stimulus, the effect of which is to excite muscular contraction, and the dilatation is still more complete under a state of paralysis which destroys muscular power altogether.

To consider the other mode of vascular dilatation; namely, by an inverted order of the contraction of the fibres which are the seat of tonic power: this action also cannot be deduced agreeably with analogy. The most analogous instances of muscular arrangement are those of the sphincter muscles; and among these we have not, that I am aware of, any example of one, the exertion of whose muscular fibres is capable of the double duty of contraction and dilatation. The action of the muscular tissue of the intestines is said to be in some cases inverted. That any fibres belonging to this structure contract in an inverted order, or in one opposite to their usual mode of action, I presume is not proved, and the supposition of such a circumstance is not requisite, since the phenomena which have given rise to the conclusion may be explained by supposing this structure to be under violent agitation, or that its natural function is greatly disturbed, or under preternatural excitement.

That a power of dilatation is possessed by the vascular system, appears to be not only not demonstrable, but upon the whole contrary to analogy. That a dilatation of this system does occur in most diseases, is perfectly obvious; and if this can be explained by no direct power possessed by the

vessels themselves, the phenomenon must be imputed to an agency derived from connected structures and related functions. The explanation which I have hazarded on this point, and which I shall hereafter continue to illustrate, supposes the vascular system to be passive in this business of dilatation: it supposes, not that the vessels dilate, but that they are dilated; that the immediate cause of their dilatation is an augmented volume, and, in inflammation, an augmented velocity (as is proved by wounds of minute vessels) of the blood. And the blood being equally passive, requires, in order that its volume or velocity may be locally augmented, the influence of another power, which has been suggested—by which blood may be derived to parts, regularly or irregularly, in health or under disease, with different degrees of force, and produce all the varieties of dilatation which are observable in the vascular system.

The dilatation of arteries is still open to another conjectural mode.—If, it may be urged, it is apparent that the arteries possess in their own structure no power of dilatation, agreeably with the rule of philosophising, which recommends the supposition of no more causes than are sufficient to account for the effect, it is to be inquired whether this increase of dilatation is not still to be explained in some way, by a recurrence to those powers of arteries which have been demonstrated and confessed?

The acknowledged powers of arteries are those of contraction; and as none of dilatation have been discovered, may not, it will perhaps be asked, the mode of their assuming an increased capacity

be by the diminution, or loss of their tonic power? The loss of the power which resists distention is, in effect, equal to the possession of one of dilatation by muscular fibres: it matters very little whether the dilatation of arteries is occasioned by a property of their structure, or whether it is permitted by the loss of a power, which otherwise resists it. But I believe that either of these provisions is equally irreconcilable with the cases to which it relates.

It is an obvious fact, that the sensible causes of determination of blood to parts are those of irritation; such as punctured wounds, other mechanical and chemical stimuli. The common, we may perhaps say, the invariable, operation of a stimulus applied to muscular fibres, is *to cause their contraction*: the common effect also of stimulation of any recognised property is the increase of its energy. So far, then, as our experience goes of the relation of a stimulus with the power of muscular contraction, it is wholly opposed to the supposition that the irritation of arterial structure should be followed immediately by a diminution of its muscular power, or by a suspended exertion of it. Irritation of a denuded artery* has not been found to produce either contraction or dilatation (although the former may occur, as remarked by Dr. Parry, from long exposure to the air); which seems to prove, in concurrence with other testimony, that the dilatation is not produced by a direct relation of a stimulus with the powers of the artery, but

* This, to be effectual, should be applied to its muscular coat.

that this dilatation follows a state which is assumed by the structure to which the artery is distributed. The order of succession will then be thus expressed: first, the infliction of mechanical injury; productive of, secondly, irritation; involving, among its other phenomena, *an increase of the derivative power of the secerning system, and of the seats of its distribution;* thirdly, a consequent local dilatation of the arteries, proceeding either from an extension of a power of affinity from the secerning system to the blood, as before suggested; or from an exertion of the same power, confined to this system, and a consequent diminution of resistance to the passage of blood in this particular direction; as minute vessels are otherwise dilated by the action of a leech or a cupping-glass. That this order of the facts is the true one is, I believe, in perceptible agreement with every example which can be quoted.

A deficient action of the absorbents tends, as is often remarked, to produce sensible phenomena, which are common also to an increased exertion of the secerning function: but I am acquainted with no criterion by which tumefaction can be affirmed to proceed from defective absorption. The preponderance of the secerning function appears to be the most general in these cases, from the circumstance that the tumefaction ceases under the means of revulsion, or under a treatment which restores the balance of the circulation.

The doctrine of determination of blood, which was stated in the beginning of this article as our thesis, assumes: first, that this determination of

blood exists in every disease; and second, that it is *the cause* of every disease. These propositions it is our business to examine; in the first place to ascertain their truth, and in the second to deduce from our analysis, if possible, the real import of the determination of blood in the production of disease.

1. That a preternatural fulness of the vessels of a part is a very general accompaniment of its diseased conditions, is matter of too frequent observation to admit of dispute. But those who have carried this doctrine of determination to its greatest extent, assume a privilege of inference which closer reasoners will be inclined to dispute. Their argument stands thus: since most diseases are accompanied by determination of blood, we infer that all are. This inference, although, as it is now presented, it appears little better than absurd, is, on some occasions, a legitimate one. If, pursuing our example, we had a sensible demonstration of a determination of blood in ninety-nine cases, and there was a total want of evidence with respect to this circumstance in the hundredth, we should *(cæteris paribus)* be justified in concluding that the hundredth case, in this respect, resembled the ninety-nine. But, if there is no defect of evidence, if we have an opportunity of convincing ourselves by the best of all testimonies, that of the senses, that in the hundredth case there is, or has been, no determination of blood, we are surely not justified in rejecting a sensible proof, in favour of an inference, supported only by an imperfect or dubious analogy. Determination of blood to a

part is shown to have existed during life, by an examination after death, when, from the situation of the part, the turgescence of its blood-vessels could not, during life, be seen. As this is the only proof of there having been a determination to such part; so, if this proof is wanting, the absence of it has a force against, equal to that of its presence in favour of, determination.

That this proof of a determination of blood to parts during life, which is obtained by their inspection after death, is sometimes wanting, must be admitted by all who are familiar with such examinations; of which I select the two following, as being connected with symptoms, for the occurrence of which a determination of blood has been most peremptorily insisted upon.

A girl, about twenty years of age, had for two years frequent attacks of hysteria; the diagnosis of which was not, however, so clearly marked, but that by some they might have been called hysterical, by others epileptic. These fits were connected with an irregular menstruation, which was always deficient in quantity, and sometimes suspended for three or four months. The treatment had in it nothing remarkable, excepting that it consisted principally of those remedies which are called antispasmodic, deobstruent, &c.; and depletion by the lancet or by leeches was very rarely employed. The treatment fell under different hands: my acquaintance with the case was only *post mortem*. After having been insensible and convulsed during a night, she died; and on the following day the brain was examined.—The com-

plexion, it should be remarked, after death, as indeed before it, was pale. The appearance of the brain was such as might belong to a person in perfect health; its vessels are certainly sometimes more turgid than in this case, where death has occurred from disease apparently unconnected with the functions of the brain. Every part of this viscus was minutely examined; there was neither extravasation, nor effusion, nor any thing like a plexus of distended vessels about the origins of nerves or in any other part.

A man, about forty years of age, of a pale complexion, complained for some time of violent pain in the head, accompanied sometimes with vertigo. He was bled, cupped, purged, &c. without relief. These symptoms continued, and he had epileptic fits at irregular intervals. He was repeatedly bled; and the blood, which at length consisted of little more than serum, always exhibited fibrine: he also had frequent and long-continued bleedings from the nose. He was bled from the temporal artery, from the jugular vein; took very little nourishment, and that of a vegetable kind; these symptoms continued, and this treatment was pursued about three months, when he died; having been previously convulsed three or four hours. The brain was found on examination certainly to exhibit no marks of congestion or vascular fulness. On the contrary, it was the palest brain I ever saw; and, as in the former case, there was neither extravasation of blood, nor effusion of fluid in any greater quantity than is commonly met with in the ventricles.

In some diseases of nerves, as in tic douloureux, the severest symptoms have existed without any evidence being afforded, by examinations after death, of a preternatural determination of blood. The same thing has been remarked in tetanus; of which the following case, which fell under my own care, is an example.

A boy, about twelve years of age, was cut in the face by a stone immediately over the middle branch of that portion of the seventh pair of nerves which forms the pes anserinus. A fortnight after the injury, while the wound was granulating, he became affected with tetanus, of which he died. The nerves forming the pes anserinus were examined, and the course of these branches was pursued in their distribution as far as possible; the dissection was then continued backwards, and the portio dura was attentively examined to the place where it escapes from the stylo-mastoid foramen: its course within the cranium was also followed from this point to its origin. This latter part was also examined carefully. The nerve was pale throughout; there was no increased vascularity, or distention of vessels about its roots, and the vessels of the brain itself were not more turgid than they may be found in subjects who die of old age, atrophy, phthisis, or any other disease the most distantly connected with the brain.

Thus it appears that determination of blood is not an universal accompaniment of disease. On this ground alone, it cannot be an *universal cause* of disease, even if it were proved that such was in other instances its character. The consideration

of its claim to be regarded as the cause of disease is the subject of our second proposition.

2. In a state of perfect health, there is an equal distribution of blood to the different structures; which distribution agrees with their vascularity, and corresponds with their organisation and functions. Now, as the blood is passive, it is obvious that the equal distribution of it depends upon a healthy state of the powers by which it is moved, and on which its distribution depends. If this is granted, (and I presume it will not be denied), it follows, that an unequal distribution is the result of an unhealthy or disordered state of the organs upon which its distribution depends. If, then, a part obtain an undue quantity of blood, this excess of blood cannot be *the cause* of the disease; because disease itself must *precede* its occurrence. Thus, the presence of an increased quantity of blood must be dismissed as the cause, since it is proved to be the effect, of disease *.

If the presence of an increased quantity of blood in a seat were imputed merely to the loss of tonic power in the arteries, the loss of this power is a

* This precedence has been insisted upon by Bichat, in treating of inflammation, in his second volume of his Anatomie Generale. He attributes the determination of blood to an increase of "organic sensibility." It was not until after this work was finished, in its present state, that I became acquainted with the opinions of Bichat on this subject; which, though on many points very similar to my own, are, however, different in some essential respects. If the views of this great man had been earlier known to me, (which circumstances already mentioned prevented,) their coincidence, as well in opinion as in illustration, with my own, would have been remarked individually, and some deficiencies of his doctrines, together with the inadequacy of some of his proofs, would have been pointed out.

part of a diseased state which precedes the presence of a preternatural quantity of blood. But it has been proved, so far as we possess evidence on the question, that the causes of determination of blood (as those of inflammation) are not likely to occasion the loss of the tonic power of arteries; but on the contrary, if they held any direct relation with the arteries (which appears not to be the case) to excite this power, tending to produce the contraction rather than the dilatation of these vessels.

But it is chiefly or wholly from the frequency of the connexion of an undue quantity of blood with local disease that its universality as a cause of disease has been inferred. It is proved, by its being preceded by disease, that the presence of an undue quantity of blood is not necessary to this effect, or that it is not the primary cause of disease. And I apprehend, that the priority of a preternatural quantity of blood to the other phenomena of disease (which, to give it the import of a cause, it is necessary to establish) cannot, in any case, be very readily distinguished.

Appealing to the order of succession, the strictest scrutiny will not enable us to perceive that the determination of blood precedes the other phenomena with which it is associated. In inflammation, for example, a sense of uneasiness or of pain precedes the visible swelling or enlargement of the blood vessels. This precedence may be distinguished by those who have suffered acute inflammation of the phlegmonous, or of perhaps any other kind. But if, in the cases of spontaneous

inflammation, it should still be denied that other symptoms of the commencement of disease precede the enlargement of the blood vessels, this precedence must be admitted where the order is, by a clear perception of the period when the action of the cause commenced, perfectly obvious to the senses. Thus, if inflammation supervene upon an external injury, the first effect of that injury, as a puncture perhaps under the nail, is to produce exquisite pain; heat, throbbing, and tumefaction succeed: but the state of irritation, denoted by the pain first consequent upon the infliction of the injury, certainly precedes all the visible phenomena which ensue, and with the occurrence of which vascular dilatation is synchronous, and also co-operative with the other circumstances of a diseased state, in the whole series of effects which might succeed to the external injury.

Thus it appears, agreeably with the strongest and most unexceptionable testimony, that the determination of blood to a part is a *consequence* of the assumption of a state of disease; that it is a part of a diseased state, which it helps to establish, and that it can be at most but auxiliary to the other causes involved in the condition of disease, in deciding its varieties and duration.

Granting then, although it is not precisely the case, that determination of blood is an invariable accompaniment of disease, this confession of its being an accompaniment distinguishes it from that which possesses the character of a cause. We may indeed be whimsical in assigning causes of disease, if the accompaniments of it are to ob-

tain this title. It was upon some such ground, that Darwin fabricated an absurd system about motions and sensations: the varieties of which are certainly as prevalent in disease as those of the determination of blood: but they are merely the phenomena of disease, or its effects, and can therefore go but little way in explaining the origin or dependences of this state. One might be more particular in his conjectures as to the import of determination; and might choose to say that inflammation is always occasioned by the colouring matter of blood, which is invariably found to be in excess in inflamed parts: another might make the same stipulation in favour of lymph; and another might be disposed to lay considerable emphasis on the increase of saline particles in inflamed structures, which may be supposed to be peculiarly annoying to them: another may impute much to pain, or heat; the sense of which is so prevalent in inflammation, &c.

If the state of disease acknowledged only one cause, the condition of disease would be the same under the existence of the same cause: and if disease show any varieties in its course, they must originate in a different state or degree of this cause, and should be, in their series, expressive only of its varieties.

But the state of disease is not in fact so simple: it must acknowledge a complication equal to that of the properties which concur to produce the structures, to support their life, and to maintain their health. This complication is shown by every diversity which is observable, whether by a gross

and general view, or by the minutest scrutiny with which the subject can be regarded. The properties which produced, support, and renew the brain are different from those which perform the same offices in regard to the structure of the eye: the same is to be said on a comparison of all the textures, the muscles, the bones, the arteries, the veins, the lymphatics, the nerves, the membranes, glands, viscera, &c. This general view exhibits the diversity of properties interested in the preservation of health. More minutely inspected, every section of structure will be found in some respects to manifest the operation of properties peculiar to this structure, and not belonging to those which are contiguous; frequently, even though these should be of the same kind. Every different arrangement in the constitution of an organ, the constituents themselves of organs, their products, the fluids compared with each other, all furnish proofs of the operation of a diversity of properties: from which immense concurrence results the harmony of a system in this particular example; as by a similar co-operation of endless constituents, the general harmony of nature is preserved. Now if the causes on which health depends are so numerous, shall it be said that disease, which is a deviation from health, and in which all these properties are liable to be interested, acknowledges only one cause? or that out of an endless variety of causes which concur to an effect, one only shall have the privilege of suffering a change? Such an explanation of disease as the one we are considering is much the

same thing, at least in propriety of argument, as to account for the function of the brain, by assigning mastication or digestion as the cause of it.

If this complicated agency of properties is necessary for the purposes of life and health, independently of the inferred necessity of a corresponding complication in disease, which is a modified state of the properties which maintain health; independently of an inferred agreement of the two conditions in this respect, the state of disease itself exhibits phenomena, which are alone sufficient to prove the diversity of the properties engaged in them, and are therefore not to be reconciled with the supposition of a solitary cause.

A part may be attacked with an inflammation, comparatively slight: in less than twenty-four hours from the beginning of the attack, in this instance, a state of gangrene may have supervened: a part may be attacked with inflammation, so far as can be judged by what we see (and we have no right to judge by what we do not see), of about the same degree, and it shall end in resolution. If the reason of these two different endings should be inquired for, we must say that the part in the former case was disposed to gangrene or slough; and that in the latter it was capable of sustaining a similar inflammation without consequent death. We mean by these two expressions "disposed to mortify," and "capable of sustaining," to designate two conditions of the properties involved in the disease, both of which are distinct from the degree of determination; which so far as could be

perceived, notwithstanding the opposite endings, was about the same. The *degree* of vitality will perhaps be quoted as furnishing an explanation; in reply to which, it is necessary only to refer to our former examination of this topic in the chapter on the pathology of Brown.

A limb may be attacked with inflammation, and in twenty-four hours it shall be erythematous, perhaps covered with minute pustular or vesicular eruptions. A limb, in another case, may be attacked with about the same degree of inflammation, shall observe a similar progress of swelling, &c. yet there shall be in the same time scarcely any degree of erythema, and no eruption of any sort. In the one case, the skin was disposed to a cutaneous eruption; which, as the inflammation or determination was apparently the same, must be imputed to a state of properties which did not prevail in the other.

Violent inflammation of a limb may terminate in resolution: a similar one, so far as it can be seen, may end in the formation of matter. Here again are animal processes of different kinds, implying a different disposition of the properties which are necessary to their respective endings. If it should be objected, that the degree of determination is not perhaps alike, although there may be some resemblance in these different cases, I would ask, is the quantity of blood too great, or too little? whichever of these is complained of, cases will be found of the same, or of greater excess, or deficiency, in which the same difference of termination has occurred.

But if the doctrine of determination is so inadequate to explain facts in these familiar cases of inflammation, what shall we say of those diseases which are said to involve specific actions? A swelling forms in a part, and it resembles in its progress perhaps a chronic abscess: on being opened, it discharges only grumous blood. Was it the large quantity of blood contained in the swelling that indisposed it to form pus? if so, how happens it that pus is formed in phlegmonous inflammation, in which the distention of vessels is as great as can be imagined? or is the quantity of blood not sufficient for the formation of matter? How then does it happen that pus is secreted on ulcers, the surfaces of which are greatly compressed by bandages? The structures, it must be replied, are *differently disposed* in the two instances. To trace the progress of our tumor: after the discharge of grumous blood, it throws out rapidly a fungus, a diseased mass. Is there any degree of determination that may be particularized, capable of producing a fungus endowed with all the peculiar tendencies manifested by the one in question?

An ulcer may be in a tranquil state, and disposed to heal—on a sudden, it assumes different actions (the blood being passive does not begin this process), and extends in one direction by quick ulceration, in another place it sloughs, and perhaps at one edge may still retain its disposition to heal. What degree of determination is necessary to produce these effects, seeing that all degrees of determination, so far as we can judge of its degrees, take

place without them? The blood in this ulcer did not begin this change. The properties of that principle which governs all vital changes commenced it. The change in these properties at least preceded any change in the determination of blood. If then these properties can change spontaneously, so as to produce certain phenomena independently of the blood, is it probable that they do not change in more respects than one at the same time; producing phenomena, namely, those for which no determination can be assigned, equally independent of the blood? or is it probable, or compatible with the history of causation which must belong to every change, that one property only of so vast a diversity should be liable to change, or to assume of itself, without influence from other causes extending the diversity of states, a different condition, and that difference to consist merely of degree? It is needless to multiply cases of this kind to show the insufficiency of the doctrine; we will, however, see how it stands in regard to some of the cases of medicine.

Two persons shall be attacked with inflammation of the lungs. The local and constitutional symptoms (the same rate of action, &c.), on the second day, shall be as nearly alike as possible. By the twelfth day, the local symptoms, except perhaps a slight cough, of one, shall have disappeared, and the pulse, from a hundred and twenty, shall be reduced to eighty: in the other, on the twelfth day, diffused pain in the chest may have ceased, some pain remains in a particular spot: the tongue of each is become clean, but the pulse of the latter,

at this time, instead of having subsided to eighty, bids defiance to any justifiable remedies to bring it below a hundred and twenty, in a minute. One patient, in short, in six weeks from the attack, is employed as usual, in a laborious occupation: while the other is emaciated, confined to his bed, spitting pus, with a pulse of a hundred and thirty. Now all the perceptible phenomena of the circulation were in these two cases the same on the second day. If here the cases began to digress in their tendencies, this is not to be ascribed to circumstances in which they were alike, but to some particulars in which they differed. The greatest sceptic in medicine will not quarrel with the term, if it should be said that a different disposition existed in the two subjects. In other words, the latent properties giving rise to states, the diversity of which was declared only by the diversity of phenomena, were not the same. If a primary disease in one subject runs into, or excites, a secondary disease, which in another subject it will not excite; or if a medical agent produces effects in one, which it does not produce in another, this can happen only because the agent in the two cases has a different relation: if the properties concerned were identical, the relation must be the same, for the plain reason that there is nothing to make it different.

Or, confining our observations to a single feature, instead of the collective symptoms of the two cases before us. The pulse in both instances, on the second day, was a hundred and twenty; what produced this rate of action in the heart? let it be

said, agreeably with the doctrine, a determination of blood to a particular part of the brain, spinal marrow, or to the heart itself. It will be granted that this determination, producing the same effects, was about the same. If, then, the determination, the cause is the same, how happens it that the pulse in one subsides to eighty, while that of the other is never below a hundred and twenty? It will be replied, the determination or cause in one ceases; in the other it is maintained. Why does it cease, or why is it maintained? Not owing to the blood, which producing the same effects, its quantity may be supposed to be relatively the same; but from a different disposition in the organs, or, in other words, from the agency of properties, which are independent of the blood, and peculiar to either case.

Four persons shall each have a pulse of a hundred, with about the same fulness, so far as it may be judged of, of the carotids, with cold feet. One shall be mad, another shall have a singing in the ears, a third shall have merely a pain in the head, and the fourth shall be neither mad, nor have a singing in the ears, nor even pain in the head. In these cases the circulation, so far as it may be perceived (and I have already said we must judge rather from what we do, than from what we do not, perceive) is about the same. Let the action of the heart, with fulness of the carotids, go through every range in some subjects from seventy to a hundred and forty, and they will neither be mad, nor have epilepsy, nor hysteria, nor will they see spectra. While in other subjects, at

almost any degree of this range above ninety or a hundred, madness, epilepsy, hysteria, false perceptions of all sorts, impaired vision, vertigo, or apoplexy, may occur. It will not be disputed but there is in these subjects a different local disposition, independently of the quantity of blood, in which respect they may resemble each other. This difference of disposition is a mere nihility, if it is not produced by the operation of different causes, or properties, in the different examples. But if the resemblance of determination in these cases should, in the way of assertion, be denied, or if it should be urged that a determination may respect only the distribution of one or two arterial branches, of which no evidence would be afforded by an examination of the trunks, this assumption would be both gratuitous and unnecessary; if, however, it were admitted, the refutation of the doctrine of determination of blood, as the cause of disease, may be confidently rested upon the other proofs before cited.

The facts which have been quoted in connexion with this subject of the determination of blood, appear to sanction the following conclusions:—

1. That a part cannot obtain a preternatural quantity of blood by the exertion of any power which belongs naturally to the arteries.

2. That the ascertained powers of the arteries, viz. their tonic and elastic powers of contraction, may be overcome by the causes which produce determination of blood.

3. That a preternatural determination of blood, although a general accompaniment, is not found

to be an invariable one, of disease, in seats where the evidence of such determination has been looked for.

4. That a determination of blood is occasioned by a local state of the structure, and cannot be produced by any action of the heart, which must be equally relative to the whole vascular system; or by any condition of this organ, except such as presents mechanical obstruction to the passage of the blood through its cavities.

5. That the determination of blood does not *commence* disease; or is not the antecedent of this condition.

6. That determination of blood is preceded by the assumption of a state of disease, which is denoted by symptoms; which state of antecedent disease, merely as it requires some term, may be expressed by the words irritation or excitement.

7. That as the recognised powers of the circulation are inadequate to account for this phenomenon, the concurrence of a function of the secerning system has been supposed necessary to this end.

8. That the secerning system consists of the terminations of arteries which separate fluids from the blood by an affinity with these fluids, which is an exertion of the properties of life.

9. That this function constitutes a power of attraction at the extremity of the arteries, which helps to carry the blood through its course, and is at once auxiliary to the heart in the circulation of the blood, and a centre of the power which makes nutrient fluids pervade the molecules of the struc-

tures; and is also capable of compelling their return through the absorbents into the system of the blood vessels; although it is probable that the absorbent orifices have a similar function, which gives additional strength to the vis à tergo, by which fluids might be otherwise conveyed through them.

If the agents of the circulation are assigned to be the heart and the capillaries, the varieties of the circulation should be looked for in one or other of these sources. If that variety which has been called preternatural determination does not take place from an impeded transmission of blood through the heart, there appears no other way in which its local occurrence can be imputed to this organ. If inflammation takes place in the face, arm, or leg, or if there is preternatural fulness of the vessels of the head, the heart has no relation with the vessels of these seats, by which it can determine to them, in particular, a preternatural quantity of blood. The relation of the heart is with the aorta; and the quantity of blood in this vessel has a common relation with the subclavian and carotid arteries, and those which belong to its descending course. If, therefore, a preternatural determination occurs locally, whether in the system of the carotid, subclavian, or any other artery, it is owing to a local state which is peculiar, giving rise to a peculiar occurrence; and not to any circumstance or condition which is common to the arteries, which are distributed to seats where no such determination is observable.

Supposing, then, an inflammation to take place in the arm, and to make the cause more palpable,

we will say, from local injury; the state thus produced would be one of irritation or of excitement, a very natural consequence of which would be an increased function of the secerning system, and hence a preternatural derivation of blood to this part, and the visible signs of determination exhibited by this condition of disease *.

That the state of irritation or excitement is one by which functions are increased, is in general conformity with our experience; it is proved by the phenomena of irritation or excitement in the brain, in the nerves, in the muscles, and in the heart; it is proved by the spontaneous phenomena of the glandular and membranous organs of secretion; and by the effects of medicines, the properties of which, being so related with, as to excite or irritate, particular seats of secretion, produce accordingly an increase of their function. Thus, the secretion of urine is increased by nitre; the secretion of the stomach, by ipecacuanha and emetic tartar; the secretion of the mucous coat of the intestines by aloes, rhubarb, scammony; the secretion of the schneiderian membrane by sternutatories; that of the lacrymal gland, by stimuli to the eye; and the secretion of the whole glandular system, more especially of the salivary glands, by the irritation of mercury, &c.

If, then, it is the common effect of irritation or

* Haller has remarked that a puncture of an artery has renewed the circulation in the vessel when it ceased. He, however, supposed the blood to be derived to the vessel by this cause, agreeably with certain mechanical laws, rather than from the exertion of the property of life here suggested.

excitement to increase the function of the parts in this condition, it is naturally to be expected, when a structure is possessed of such a state, that the function of its secerning system, which enters minutely into every section of the structure, should be augmented; and hence the preternatural determination of fluids to parts under this condition.

But in this particular instance, the cause of increased determination has relation with a final purpose. If the life of a seat be under preternatural excitement, agreeably with analogies in physiology which need scarcely be particularized, it is to be concluded that its consumption is more rapid than usual; that its assimilation must therefore be proportionally rapid; and that the supply of the material from whence it is derived, namely blood, must be proportionally increased.

This state of irritation is a common one in local disease; the order of occurrence is obvious when it is produced by external causes; and the prevalence of local determination of blood in diseases is commensurate with that state of excitement to which it has just been imputed.

But this state of excitement and preternatural increase of function appears to be a circumstance, like the determination which is the index of it, which is common to very different states of disease. It is alone, therefore, no source of the explanation of the phenomena of disease; it being, as we have remarked of the determination of blood, an accompaniment of a great diversity of diseased conditions, which are liable to involve all that

complication of properties which has been said to concur to the maintenance of life and health.

In almost every case of disease which can be cited, a different disposition is shown. It is the common effect of most diseases to produce derivation of blood to their seats, the continuance of which is *not dependent* upon the presence of a preternatural quantity of blood, but, like the other phenomena which characterize respective diseases, *upon that condition* which constitutes the disease.

Thus, there is agreement in the simple circumstance of a local preternatural quantity of blood in nervous headachs, as they are called, in mania, in epilepsy, in hysteria, in carus, in apoplexy; in inflammations of the phlegmonous kind, which end in resolution; in inflammations of the same kind, which end in suppuration, and in those which end in gangrene; in erysipelatous inflammations, expressed only by tumefaction and redness of the skin; in those of the same kind, which form vesicles, and in those which end in mortification; in gout which ends in perfect resolution, in gout which deposits chalk stones; in rheumatism which affects muscles or joints severely, and leaves them with their motions unimpaired, and in rheumatism which produces contractions of the limbs; in the inflammation of the skin which produces variolous suppuration, in that which produces the vaccine and syphilitic poisons; in the seats of scrophula; and in those of cancer.

As the determination in all these, and in other cases, is dependent upon the disease; so it is ob-

vious that a treatment which entertains exclusively the object of correcting the determination, cannot succeed in this respect, unless it also has *a curative relation with the disease.*

The relation of blood with the state of disease is to maintain (by maintaining the life of the part) the condition which this principle has assumed. The concurrence of blood to support disease is forced by the state of disease, which involves the derivation of blood by the additional energy or increase of function, which is an effect of disease. It is a result of this preternatural derivation, that the small arteries of inflamed seats sometimes bleed, when cut, for hours, when others of the same size would not bleed five minutes.

Yet it is possible that disease may occur in a seat which does not involve or affect all the properties of such a seat; and consequently, that the function resulting from such unaffected properties will not be augmented. This separate affection of properties in a structure where they are so intimately allied is, as one would expect, of rare occurrence; and hence the infrequency of disease without increased energy of the secerning extremities of arteries, and a consequent preternatural fulness of blood vessels.

But in the diseased condition of parts, that some properties may be excepted is indicated by the occurrence of diseases without visible increase of blood in their seats; as well as by the fact, that persons who are well nourished are frequently dyspeptic in an extreme degree; insomuch, that I know persons who, from their general appear-

ance, would be quoted as models of health, whose tongues have for years been white and much furred, and who suffer great inconvenience from a disordered state of the stomach; which seems to prove that the dyspepsia may engage other properties of this viscus, and leave unaffected those which are specifically subservient to digestion.

But the cases quoted, as exemplifying the existence of disease without determination, are not all of them unexceptionable; as in tic douloureux, the seat of the disease may be in the filaments of nerves which are not to be traced by dissection, while the examinations have been made of the trunks; and in tetanus, the seat of the disease may be in the muscles, while the examinations, from seeing that affections of the brain, in other instances, originate spasmodic diseases of the muscles, have been of this viscus. Thus, of the instances in question, it may be said, that the preternatural fulness of the vessels is sometimes not discoverable, by reason that the energy of function which should occasion it is not increased; and that at other times this turgescence of vessels is sought for in parts which are not the seat of disease.

It appears that there are not only cases in which there is no preternatural derivation of blood, but that, in some instances, disease is associated with a deficient energy of the function to which its presence has been imputed. It is not improbable but that in some constitutions, peculiarly cold and apathetic, such as every one must have met with in his practice, suspended menstruation may be

imputed to a disordered state of the uterus, which, so far as the function we are considering is concerned, is the reverse of one of increased energy: and more obviously, organs and limbs that are paralysed, as the optic nerves, arms, legs, &c., so far from being distended under this diseased condition by a preternatural quantity of blood, contain less than their ordinary quantity, which is denoted by the diminished fulness of their vessels, and the general shrinking, or wasting, as it is called, of their structure. How far other diseases, as some forms of atrophy, may be justly imputed to a similar diminished energy of function, and therefore supply of blood, affecting parts subservient to nutrition, I presume we want facts to determine; or at least it is requisite that the import of those we possess should be more accurately discriminated by future observation.

By a reference to examples it must be admitted that the preternatural derivation of blood is a circumstance which almost invariably accompanies diseases, whose seats are submitted to our inspection. The local increase of blood may be said to belong to irritative, inflammatory, and specific disease. The first is exemplified in the determination to the head, which succeeds to anxiety, excess of reflection, the excitation of the passions; to sudden impressions on the senses, or to the stimulation of alcohol; perhaps also it is exemplified in certain states of the mucous and serous membranes, in which there is turgescence of vessels, with slight sense of irritation, increasing or changing their natural secretions, as those of the throat, lungs,

tunica vaginalis testis, and of the peritoneum, &c. The presence of local increase of blood is one of the characteristics of inflammation; and, in this instance, need only be adverted to, in order to connect it with a state which, by the combination of a greater degree of pain, by a sense of heat, throbbing, and the existence of fibrine on the surface of the crassamentum, is distinguished from the derivation of blood which is produced by more simple, or less severe forms of disease, and which are therefore said to be those merely of irritation. The presence of a preternatural quantity of blood is also sufficiently obvious in our third class, or in those diseases which may be called specific: such as cancer, fungus hematodes, the various kinds of tumours, in diseases of the skin, and in those which form preternatural depositions, as of calcareous matter in enlarged glands, in encysted tumors, or in tubercles, perhaps urinary calculi, and the chalky depositions of gout, &c.

The uterus furnishes examples both of irritative and inflammatory derivation, as well in connexion with its natural functions as in its diseased states. Thus, we have in this seat, menorrhagia, the derivation by utero-gestation, in which state the blood affords the evidence of inflammation, polypi, fungous excrescences, and schirrus.

Of the succession of increased local quantity of blood to the disordered state, which, for the sake of preserving a familiar term, has been designated as one of irritation, the case of utero-gestation furnishes a striking and additional example. We observe this organ, previous to impregnation,

of an inconsiderable size, and its vessels small. The irritation occasioned by the presence of a fœtus derives blood for the purposes of supporting additional life, and to furnish the material of additional nutrition. This action is continued here, as before observed of irritation in other seats, with reference to a final purpose, until the organ of inconsiderable magnitude becomes one of immense capacity, and until its aggregate area of vascularity is increased a hundred fold.

But it may be observed in objection to the theory that the increased presence of blood is owing to an increase of the function of the secerning system, if this were the case, how happens it that there are considerable determinations of blood without secretion? Thus in the brain, for example; the determinations which accompany hysteria, insanity, epilepsy, &c. rarely are followed by serous effusion and apoplexy. In the inflamed state also of a serous or mucous membrane it is usually dry, and secretion does not occur until the violence of the inflammation has begun to abate.

In reply to this objection, it must be remembered that the power of affinity, said to belong to the secerning system, communicating with the arteries, is only with certain fluids of these vessels; and that the object of this function is to distribute a fluid of nutrition to the minutest components of the structures. The vessels, then, which subserve this function, are not necessarily those which open on the surface, but those which pour their fluids into the interstices; the minutest molecules of which they permeate, and, after having accom-

plished their share in the business of nutrition, renovation, &c. are forced by a vis a tergo, or drawn by another action of the affinity of life, into the system of the absorbents. Those properties of the structure which are most under preternatural affection, or in the highest degree of excitement, will derive and consume the largest proportion of the materials which are supplied to them, by the increased energy of the function, by which they are separated from the blood; and an order of vessels, opening superficially, whose business it is to effuse fluids on the surface, and to derive from those which supply the components of the textures, will be deprived of fluids, so long as the excitement of other properties shall preponderate; and will relieve the internal structure, or diminish the fluids which permeate them, when *their* function is either additionally excited, or when the excitement of other properties is diminished.

Thus, it appears that the life of a seat having assumed the disordered condition which we have termed irritation, a corresponding increase of the energy of the function of separation is necessary to supply the augmented rapidity with which, under this state, the materials of nutrition are consumed; that, therefore, the increased function of the secerning vessels communicating with the arteries is almost a common, or invariable one, to the states of irritation; but that the increased function of the secreting vessels which open on the surface, and which can have no connexion with the increased demand for the materials of nutrition, arising from an excited state of the

structure, assume an increase of their function only in the progress of affection, or else when the state of internal excitement (and consequent demand for nutrient fluids) has begun to diminish. Thus, also in inflammation of the liver, or of the kidneys, the excretory function of these organs is diminished: the balance of excitement being not in favour of that order of vessels whose business it is to excrete, but of those which are to feed the diseased condition, the properties of which have their seat in those structures whose demand is for the materials of nutrition.

But the secerning function of the vessels of excretion of a structure, are sometimes the seat of irritation or preternatural excitement, and we find diseased states accompanied by determinations, expressed by an increase of the secretions proper to glands or membranes, as those of the liver, kidneys, peritoneum, or internal coat of the intestines.

It appears not clear in agreement with what law the additional derivation of fluids is made under states of irritation by the system of capillaries, communicating with the arteries. The alternatives are, that these vessels, forming a part of the structure, participate in the irritation which is followed by increase of their function, and therefore increased derivation of blood: or that their function is increased from a relation of nutrition with properties under preternatural excitement, the result of which relation is to supply to those properties materials, in proportion to their consumption and consequent demand for them. Thus

the appetite is said, cæteris paribus, to be in proportion to the demand for nourishment in the system generally; not in proportion to the ingesta, under the accumulation of which the appetite is, in the atrophy occasioned by obstruction of the mesenteric absorbents, but partly allayed.

But whatever order of secerning vessels is affected, whether those of excretion, or those subservient to nutrition, the derivation of fluids by the secerning system communicating with the arteries either participates in the excitement, or else is excited to afford a supply proportional to the demand; and hence there is in both instances, if the distinction be admitted, a preternatural derivation of blood.

Indeed, although this power of derivation, which has its seat somewhere in the extremities of arteries, is, I think, justly inferred, yet we cannot limit the seat of this power: we observe that its final purpose is nutrition, comprehending the support of life and organization; for which purpose the fluids separated must pervade component particles; and as these minute spheres are those of their action, and at the same time the seat of change or disease in the properties allied with particles of the structures, we can scarcely say whether the power of separation is extended from these minute spheres to the places of communication of secerning extremities with the arteries, or whether the latter constitute wholly or in part its seat. Our facts only go the extent of proving under the state of preternatural excitement and consumption of the life of minute spheres, a corresponding in-

crease in the power which separates fluids from the blood, which latter is derived to parts in this condition in preternatural quantity, by an increase of the power which is habitually exerted *.

Connecting the distribution of blood with the powers of life, in a ratio to the excitation of which blood appears to be derived to respective seats, we shall, I apprehend, be furnished with an easy explanation of those irregularities of the circulation which occur frequently towards the extinction of life; and sometimes in the conditions of disease which may approach partially to such extinction. Thus a few hours preceding death we may, not very infrequently, find a distinct pulse in the carotids and temporal arteries, and no pulse in either wrist; and in cases of disease which have ended in death, and sometimes in recovery, I have often known the pulse rather full than otherwise in one wrist, and to be perceived but faintly in the other. These different states of the circulation agree perfectly with the supposition that the presence of blood in a part is determined by the influence, and dependent on the condition, of the life of such part.

No case can tend more to confirm the truth of this explanation, so far as the testimony of a single fact goes, than one mentioned by the late Dr. Parry †. This experienced and intelligent author observes, that he has seen a total loss of pulse in one arm,

* The chapters on nutrition, secretion, and absorption, in Indications which relate to the Laws of the Organic Life, may be advantageously consulted in connexion with these views.

† Inquiry on the Arterial Pulse, p. 130.

with *coldness*, but complete power of motion in that part; while the other arm was *warm*, and possessed a perfectly good pulse, but had lost all power of voluntary motion. The circumstances of coldness in one arm with loss of pulse, and warmth in the other with preservation of the pulse, indicate, in common with the local cessation of the pulse shortly preceding death, that the circulation was in a very low state where the powers of life were reduced, and nearly natural where the powers of organic life were but little impaired. I am aware that there is here a choice between two causes of this circumstance, as we shall presently notice. The loss or preservation of voluntary motion does not appear to be materially connected with this argument.

The case of a female is also related by Dr. Parry, who, whilst walking about the house, lost the pulse of one arm; "a few days afterwards she died suddenly. The whole course of the artery to the aorta was carefully examined; but *no deviation from the healthy state* could be perceived in it." This circumstance Dr. Parry imputes to the contraction of the arteries which precedes death: but it seems more probable that the contraction of an artery arises from the diminished distention, or resistance to its contractile power, produced by a diminished derivation of blood, than that this power should be increased when all the other powers of life are declining; and that in the reverse state, in inflammation by external injury, its power of contraction should be diminished, when all the other powers are preternaturally excited.

Dr. Parry, referring to his experiments, observes, that in conformity with them "the approach of death produces a great degree of contraction in arteries." But there is no experiment quoted in which the contraction, so far as sensation may be admitted as evidence, is so considerable either preceding or after death, as is manifested in the small wiry pulse which is felt in some cases of visceral inflammation. Indeed, according to Dr. Parry's own experiments, the contraction of arteries on the approach of death is not a certain occurrence; on the contrary, as this state was approached in the following instance*, the artery appeared to become more dilated. In the experiment in which a sheep is killed by successive bleedings, following each by a measurement of the artery, at the fifth bleeding the artery was only $\frac{127}{400}$ of an inch; at the sixth, $\frac{140}{400}$; at the seventh, $\frac{141}{400}$; at the eighth, immediately preceding death, $\frac{145}{400}$ of an inch. Thus, towards the approach of death the circumference of the artery *increased* $\frac{18}{400}$ of an inch.

If the system of the distribution of one considerable artery is preternaturally excited, it will derive blood to this seat in preternatural quantity; and if the affection of this artery is exclusive, and its tendency not counteracted by a general diathesis, the system of contiguous vessels will be deprived of their blood by such local excitement and preternatural derivation. This, it may be presumed, is the case in enteritis. If the powers

* Inquiry on the Arterial Pulse, p. 46.

of life are reduced, or nearly extinct, in one part, and not in another, the derivation of blood will be diminished in the former, and not in the latter; and the two states of the circulation will be indicated by loss of pulse in the one, and its continuance in the other. When the action of the heart is no longer aided by the function of the extreme vessels, the stream of blood is small, perhaps not sufficient to fill the arteries, and is propelled through them only by a vis a tergo; and the sense of a pulse is no longer afforded, because the heart alone is not capable of communicating a distinct impulse to a column of blood, which receives no aid from that power of derivation, which in health lives and is exercised in the minute seats of the distribution of the secerning extremities. And these varieties in the acquisition of fluids are no less exemplified on that large scale when the state of the circulation is compared in two limbs, than on the more minute one, when in the inflammation of an excretory organ, as the liver, the system subservient to nutrition robs that which is subservient to excretion. If these facts are short of affording proof to the theory, they at least furnish examples of agreement.

The increased derivation of blood by the relative power of the extremities of arteries with this fluid, is not the only way in which the state of irritation secures to itself a supply of its material proportioned to the increased demand. In most local diseases the action of the heart is quickened; and thus the supply of renovated or arterial blood is rendered more rapid.

This accelerated action of the heart frequently appears to be synchronous with the determination of blood in local diseases: in some instances, it appears to be the primary cause of disease. But if the succession be scrutinized, I apprehend it will most commonly be found that the state of irritation in the seat of local disease precedes the accelerated action of the heart, and that the cause of this action is a sympathetic extension of the state of irritation from its seat, to this organ. This succession is not obvious in all spontaneous diseases, though perhaps it is in most: but it is perfectly evident where the phenomena are produced by external injury. Thus, fever succeeds to the infliction of mechanical injuries, as those of wounds and fractures; and the rapidity of the pulse, or of the action of the heart, increases, or abates, as the degree of local irritation is augmented or diminished.

By this remark it is by no means intended to preclude the heart itself from the ability of originating disease, which may display itself in this organ by an accelerated action; we cannot make such an exception in favour of any organ, or structure; but if the action of the heart, independently of any distant source of irritation, should be quickened by causes which have relation with this organ, all other parts of the system partake alike of this accelerated supply of arterial blood, and no one part will suffer in particular, unless it is disposed to assume the state of irritation from the influence of such an exciting cause; or else in the same way, as when, being already in the state of irritation, this state is extended to the heart, and

the accelerated circulation has the force of a re-agent.

It is owing to the general dependence of the action of the heart upon the local disease, that the pulse affords so common a criterion of the degree or importance of the local disease. This connexion or dependence is so general, that the danger arising from local disease may most commonly be correctly appreciated by the rapidity of the pulse. Thus, for example, I should have no fears about the event of a case of fever, of the type of synochus, or of typhus, in which, with determination to the head, and perhaps delirium, the pulse did not exceed a hundred and ten, or perhaps even a hundred and twenty, in twelve hours after the rigor. But I should be very apprehensive of a fatal result, if, in twelve hours from the commencement of the symptoms, the pulse reached a hundred and forty in a minute. The same remark obtains generally in acute pulmonary and hepatic disease, in affections of the head, &c.; and to a great extent in chronic diseases. I say, this observation is true generally, but not invariably. It must have occurred to others, as well as to myself, to have met with cases which have terminated fatally, in the course of which the pulse has not greatly deviated from its ordinary rate: but this circumstance is comparatively so rare, that it alone may, on some occasions, create apprehension of the danger of anomalous or uncharacterized disease, because the disturbance of the pulse does not correspond with the other symptoms of serious disease.

Thus it may be said that the ordinary relation

of the heart with local disease is to participate in the excitement, which the accelerated circulation in turn helps to maintain, both by an accelerated supply of arterial blood, and probably also by the mechanical influence of a rapid circulation upon parts under a state of preternatural irritation and sensibility; in those examples in which such a connexion subsists, the importance of the local disease will be denoted by the pulse; and its increase or diminution expressed by a corresponding increased or reduced frequency of the pulse.

It frequently happens in acute inflammatory disease, as of the lungs, liver, &c. that as the rapidity of the pulse abates, the superficial secretions commence, or are increased. The diminished action of the heart in such cases proves a diminished energy or excitement in the seat of the local disease which we have supposed to be in those minute spheres of the structures which are supplied by the nutrient secerning system, an abatement in the energy of which admits an exertion of the function of the excretory vessels, which were before deprived of their fluids by the preponderating energy of those belonging to a connected system. I am not aware of any other way in which the facts just adverted to can be connected with any physiological distinctions, than by supposing that the excess of excitement preponderates in one order of vessels, and that the function of another order of vessels comes to be exerted, when this excess is moderated, and the energy or excitement of the two systems rendered more equal. That the seats of the internal distribution of vessels should be

more liable to this excess in acute disease, or should have the greater importance, one would expect from the circumstances that they subserve to the support of that life, of which disease is a modified condition, and that this system of vessels is also infinitely the most extensive.

It has been remarked, that the distant local irritation, most commonly and obviously in the case of external injuries, precedes the accelerated action of the heart: it has also been admitted, that the accelerated action of the heart sometimes precedes the distant local disorder. Instances of both are furnished by affections of the head. Thus, if the brain is severely exercised in continued thinking for four or five hours, there may come on gradually sense of tightness over the eyes, which will increase to violent pain in the front of the head, with perhaps sense of throbbing, and the pulse at first not exceeding its ordinary rate of seventy-two in a minute, may get gradually up to eighty-four. The precedence of irritation in this case is evident. On other occasions, violent exercise, or the stimulus of wine, might produce a pulse of a hundred, with general sense of throbbing, to which will succeed pain in the head, similar to that just described. If pain in the head succeeds to this quickened action of the heart, this action is commonly maintained by it: if pain does not succeed to a circulation greatly accelerated by, perhaps, violent exercise, the heart soon recovers its usual rate of action; or, as is not uncommon, the pulse is perhaps much lowered by exercise, after the body has been a short time at rest.

The inadequacy of a rapid circulation alone to produce disease is also indicated by a most anomalous, although habitual frequency of the pulse in some persons. I have known a young man of twenty, who appeared to be in a state of perfect health, whose pulse was, I believe, never below a hundred and ten or a hundred and twenty, and I have felt it a hundred and forty in a minute, when he has been under no unusual excitement. This state of the circulation was unaccompanied by local disease. I have also known a gentleman, apparently in the best possible health, about fifty years of age, whose pulse, as he assured me, had not for some years been under a hundred and forty, and I have myself felt it exceeding this rate, without being attended by any symptoms of disease.

If a priority or independence of affection is, on some occasions, conceded with respect to the heart, it is next to be inquired to what states of the arteries the *origin* of disease might be imputed, their agency in this way having been denied in the general instances of preternatural determination of blood.

The only ascertained powers of the arteries are those of elastic, and of vital, or tonic contraction. The varieties of these powers must be chiefly those of degree, as, supposing an ordinary or mean state, of increased or diminished elastic, or increased or diminished tonic, power.

These are the possible varieties; but it is to be presumed the possibility of these varieties of the elastic power is rather inferred from distant analogies, than so far ascertained that their examples may be specified. No case occurs to me in

which the elastic power can be said to be increased, unless this should be supposed in some contracted states of arteries which are rather assumed than demonstrated, and which have then been attributed chiefly to the exertion of their tonic power. The examples of the diminished elasticity of arteries are principally furnished by states of these vessels connected with aneurism. It can scarcely be doubted that the elasticity of arteries is diminished by ossification: it may possibly be the case also in aneurism, whether consisting of a dilatation of all the coats of an artery, or of a rupture of the internal one. We know of no cause directly related with the elastic power of arteries by which this power may be either increased or diminished. We have reason to think it a property of matter dependent upon attraction between the particles of substances; but we can neither specify, with any degree of certainty, the nature of this attraction, nor particularize the agents, nor describe the modes by which the force of this property may be modified. We can, in short, say little more about it as it is connected with disease, than that the structure of arteries is liable to the deposition of phosphate of lime, in consequence of which they may become brittle or inelastic.

The examples of increased and diminished tonic power of the arteries in disease are also rather supposed than demonstrated. As, however, the tonic power is one which is ascertained to belong to arteries, there is no reason why this should not be sometimes primarily affected in disease as well

as any other property allied with the structures. Whether or not this power is diminished in those cases of determination in which there exist, in a great degree, those symptoms which people have chosen to consider as signs of debility, may be merely suspected, but cannot, I presume, be proved.

That a denuded artery exhibits different degrees of contraction, under the circumstances of long exposure to the air, a separation from all its natural connexions, or under the influence of excessive depletion, furnishes but a very indifferent proof of the participation of varieties of this power in disease: the condition of a vessel under such circumstances is so perfectly unnatural, as scarcely to afford a presumption of an analogy from which phenomena, connected with its ordinary state, may be deduced.

There is no example of determination of blood, without mechanical impediment, consisting merely of disordered function, in which the dilatation of the arteries may not be imputed to the increased local derivation of blood; and that this explanation is to be preferred to that of a primary loss of tonic power in the arteries is merely in agreement with the facts, that irritation, where the order can be taken account of, precedes the dilatation of the arteries; that irritation applied to denuded arteries has not the effect of increasing their dilatation; and that it is contrary to analogy to suppose that a stimulus, which acts upon a contractile power, should diminish its power of contraction, or should produce a state of relaxation, instead of one of contraction.

We observe also that, under many natural circumstances, the dilatation of the arteries of a part is as considerable as under disease, and yet none of the phenomena of disease are produced by it. Thus, if many persons in health were to run a mile in five minutes, the pulse in the carotids would be greater than it is felt in the severest affections of the head, and the pulse in the digital arteries would be as full as in the case of a whitlow. The same is to be observed with respect to the rapidity of the circulation, which, in this case, would exceed that which occurs in many severe inflammatory diseases. But if a part has assumed a diseased state, then this increased dilatation of the arteries, and the increased rapidity of the circulation, although they cannot produce, maintain, and are sensibly felt to augment, inflammatory disease; or, if there were predisposition in any organ, as the brain, for instance, to the state of irritation, which predisposition is a state of disease, inferred, though not declared, by symptoms; or, if there were inflammation of the finger; then the causes which tended to furnish these parts with an additional supply of blood, or to impel it through their organization with preternatural velocity, would, in the one case, produce an irritation or state of disease in an organ disposed to the excitement of it by such a stimulus; or, in the other, it would augment the irritation which already existed, and which was expressed by symptoms.

Thus, also, it happens in those who are disposed to disorder of the head, that in such persons a neck-

handkerchief can scarcely be worn with the ordinary degree of tightness, without occasioning sense of irritation, pain, and throbbing in the head; while those who have no such predisposition are not thus affected by an impeded return of blood from such a cause, with which exercise, or position, may also concur, without being productive of the slightest inconvenience.

It seems also doubtful whether the loss of tonic power is a cause either predisponent or immediate of aneurism. This disease seems rather to be connected with structural changes in arteries, which are the result of a previous process of disease, in which, most probably, many properties are engaged. The rupture of arteries, as those of the brain, occasioning apoplexy, or those of the lungs, occasioning hæmoptysis, appears to have a possible connexion with the loss of their tonic power. But here again, the loss of such tonic power is not alone sufficient to explain this circumstance. In the case of apoplexy the only mode we have of judging of the dilatation of the arteries of its seat, is by an examination of the carotid and temporal arteries. In some cases of apoplexy these are certainly very full; in others, and in fatal cases, I have known apoplexy to occur when the fulness of the carotids has been much less than in many affections of the head which are called nervous, or than in conditions of these vessels, as after violent exercise, which have been accompanied by no symptoms of disease. I have also known cases of periodical asthma, occurring, perhaps, once or twice in a year, in which the respiration has been

for some weeks most laborious, with great turgescence of the vessels of the head and face: it can scarcely be doubted that the dilatation of the bronchial vessels was much greater in these cases, than when, without impeded respiration, merely preceded, perhaps, by a slight sense of heat in the chest, a person's mouth is suddenly filled with blood, which may continue to be coughed up for many days, with a respiration so easy as to preclude all comparison with the evidences of congestion and vascular dilatation furnished by those other cases, in which expectoration of blood did not occur. Perhaps, it might be observed, the congestion producing asthma, in the cases above alluded to, did not exemplify mere vascular dilatation; but that the symptoms were connected with disease of the left side of the heart, impeding the return of blood from the pulmonary veins. Such a remark would properly belong to the citation of some such cases; but in these, the interval, perhaps, of ten months between the attacks, which was a period of the most perfect health, will preclude the supposition of a disease of the heart; besides which, the cases alluded to were those of severe catarrhal affection, and constituted an attack of acute disease, altogether different from the symptoms, more or less habitual, which are produced by organic obstruction in the heart.

It appears, therefore, that the rupture of blood vessels is not dependent wholly on the loss of their tonic power; but that it happens from a state of the artery, which is an accompaniment to a more general condition of disease, extending to

the seats of capillary distribution; to the affection of which the increased derivation of blood, which obtains in the cases of the rupture of the arteries, is principally to be imputed. It would be difficult to say in what precise condition of an artery the change consists which disposes it to rupture, or how far it might be a grade or modification of the state which, in other instances, disposes to the ordinary forms of aneurism. Although, agreeably with analogy, it may, perhaps, be admitted, that the loss of tonic power may concur with the other causes of aneurism, yet I presume its share, or importance, in this effect cannot be defined. If it were allowable to hazard a conjecture, supported only by a tissue of theory, I should rather be disposed to look for the cause of the rupture of a blood-vessel, among the properties of life, which first assign, and afterwards maintain, the places of organic particles, than to any power of contractility which may be supposed to be exerted on a vessel, the formation and preservation of which must be imputed to agents, justly inferred, although not holding a relation with the senses by which they might be recognized; but which are, nevertheless, concluded in animals, to be superadded to matter, which they govern, and dispose of, in relation to the final purpose of maintaining life.

If the dependence of the integrity of arteries is not upon any power of contraction with which they are furnished, neither does it appear to be wholly upon the quantity of blood they contain, although this, like the loss of contractile power, may concur to the effect. In the cases of con-

gestion just alluded to, and in many others with which our experience furnishes us, the vessels are observed to support the greatest volume of blood of which they are capable, and which may be much greater than there is any evidence of in the cases of spontaneous extravasation, without rupture. We see apoplexy, also, supervening upon the close of three or four weeks active depletion by repeated bleedings, purgatives, and starvation, when its occurrence can certainly not be attributed to an augmented volume of blood, which is, in fact, preternaturally diminished.

So far, then, as we are capable of discriminating the evidence of the facts, it appears that the immediate dependence of the rupture of an artery is upon a state of disease, the precise nature of which cannot be ascertained: but it is supposed to have relation with the causes which govern the cohesion of the particles composing the fabric of the vessels; that the loss, or diminution of the tonic power of the artery may, if indeed it occurs at all, be directly connected with the other properties upon which the circumstance more essentially depends; and, if not directly connected, may be auxiliary to the rupture of the artery, by permitting its easy dilatation, and an increased distention, by the presence of an increased quantity of blood. The additional impetus of the blood, also, given by an increased action of the heart, may have a similar importance. These may be said to be concurring causes to the rupture of an artery; and another cause of the same kind, and perhaps one of more general efficacy, is that state of

disease or irritation, which has been said to operate by deriving blood to its seat; and the supposition of which, as a general cause of preternatural local quantity of blood, is more agreeable with the whole of our experience on this subject than that of any other.

Although we are not possessed of any examples in which rupture of blood-vessels, or even determination of blood, is to be imputed to diminution or loss of the two recognised powers of contraction belonging to arteries; there are cases in which the disposition to rupture alone seems to ensure this effect, without the aid of the concurring causes just mentioned. Thus, for example, I have known fatal sanguineous apoplexy occur which has not been accompanied by any unusual fulness of the carotids; the face was pale both before and after death, the circulation rather low than otherwise, and the attack was not preceded by any pain in the head, which indicated the existence of that state of irritation to which we have assigned generally the preternatural derivation of blood.

As it is doubtful whether the loss of the tonic power of arteries is ever a primary affection, or indeed in what cases it ever occurs, the same is to be remarked, in the ordinary phenomena of disease, with respect to a preternatural exertion of this power. We know of no spontaneous example of the preternatural contractility of arteries: the existence of a diminished volume of blood in parts will admit a preternatural contraction, as by the reduction of an antagonist power; but the dimi-

nished quantity of blood in a part has been shown to be owing either to the increased derivation of it by a contiguous or connected system of vessels, or else by a diminished exertion of the causes of derivation, connected with the capillary system, which operate perpetually for the supply of a material to the structures, for the renewal of their life, and the renovation of their organic particles. The influence of cold may, perhaps, be quoted, as exemplifying a cause of vascular contraction. The fact that it is so is sufficiently proved by the experience derived from amputations in northern latitudes, where the thermometer has been some degrees below zero. But our examination refers to spontaneous internal changes, which certainly can derive no analogical testimony from the effects of an atmosphere below zero, on a denuded artery.

The more diffused influence of cold, applied to the natural surfaces, may, perhaps, be cited as a cause of vascular contraction. Without denying that vascular contraction may be produced by such external influence, it must be remarked, that the relation of cold may, in this case, be to reduce the natural state of excitement and consequent derivation of fluids to the surface, as a greater degree of cold, and a longer exposure to its influence, is found to render the life of parts altogether extinct.

Various medicines, such as bark, zinc, steel, the mineral acids, &c., are given internally, with the view of exciting the contractile power of arteries. They may be given with this view, but it does not follow that their operation is expressly of this

kind. On the contrary, it may be apprehended that they are as likely to diminish vascular dilatation by a relation with the causes which produce it, as by any direct or exclusive operation upon the tonic power of the arteries; which operation, if sufficient to overcome the countervailing force of a diseased state which occasioned such preternatural dilatation in one seat, might contract prejudicially the other systems of vessels which are equally exposed to the same agency, and in which there is neither a deficient contractility to be supplied, nor a cause of preternatural derivation of blood to be overcome.

As the blood is passive, we have seen that the irregularities of its distribution are owing to local states of function connected with the vascular system. Excessive repletion, or the production of an excess of blood, by good living, and good digestion, is commonly regarded as a cause of disease, such as of apoplexy, and of inflammations, in which the proofs of determination of blood appear the least equivocal. But as such excess of blood has the same hydraulic relation with all the vessels, so there is no reason why one part should suffer from excess of repletion rather than another. Yet the fact is well proved by experience, that excess of blood is frequently productive of disease.

When local disease occurs in connexion with excess of repletion, it is either because the organ or structure in which it occurs has a previous functional derangement, excited by other causes; or else that its predisposition, which may, perhaps, have the same origin, although of a more

chronic kind, is so related with the excess of blood, that the latter may become an exciting cause. Instances of the first are those cases of apoplexy which occur in the subjects of excessive repletion, when the brain is under the excitement of strong mental labour, of sudden and extreme anxiety, or of the more tempestuous passions. If the state of excessive repletion had, in these cases, been reduced by a proper training, consisting of exercise and abstinence, it is probable the same excitement would not be followed by the same effects. Instances of the second, or of excess of repletion producing local disease, from a local predisposition to excitement from this cause, are furnished in the examples of persons who suffer an acute inflammatory attack of the liver, for instance, in the cure of which they are exceedingly reduced: the disease having ceased, they return to their usual habits of living, and get apparently into very high health, and full of blood; and having attained a state of great repletion, the same seat, in which the predisposition existed, becomes again excited; they get another attack of inflammation of the liver, followed by the reduction of treatment, &c.; this reduction followed by repletion; repletion again exciting the seat of predisposition, whether the liver or any other part; and these successions of local disease following repletion, repletion following reduction, and again exciting the predisposition to local disease, I have known to occur in some persons with great regularity; and generally, though not always, the predisposition keeps steady possession of the same organ,

until death is finally occasioned by structural disease, or disorganization.

But supposing there to exist no predisposition to local disease, it is probable that repletion would be compatible with health, although the degree of it may be so considerable as to constitute a state of danger, from the possible supervention of local predisposition from other causes. It is probable that the further excess of blood would be limited by diminished appetite, which appears to be in proportion to nutrition rather than to the quantity of the ingesta; or, if notwithstanding this hint, that the system was already abundantly supplied, ingesta should be still forced into the stomach, agreeably with the artificial and depraved habits of mankind; it is then probable that the excessive formation of blood would find a limit either in defective digestion, or in the diminished function of the mesenteric absorbents. But supposing both these provisions or checks to fail, a general diathesis of excitement, expressing itself, perhaps, by fever, would ensue, from a general stimulation of the seats of life, which, disturbing its natural relations in connected spheres, would, most probably, in its course give rise to local disease.

It seems that a frequent, perhaps a general provision, against disease from excessive repletion, is the accumulation of fat; which, if this is the case, is a product of disease, and proves that both a common seat of predisposition and a provision of security against the effects of excessive repletion, are in the cellular membrane. Thus, if you confine an animal, as an ox, in a stall, allow him

no exercise, and subject him to a preternatural feeding, this disposition in the cellular membrane manifests itself, and shows that the provision of nature against the febrile and visceral disease, which might otherwise ensue from such excess of repletion, is in the excitement of the function or properties residing in the cellular membrane, by which fat is assimilated. Thus, also, it happens with two men who are gluttons or drunkards; the provision in the cellular membrane against the effects of these habits shall operate in the one, and he shall enjoy good health, and from his general appearance, perhaps, be quoted as a model of this state: in the other, if he does not get fat, the habits of excessive stimulation and repletion will kill him: he will have constant fever, and, in the course of it, local disease, at first merely inflammatory, but running eventually into one of disorganization.

If the provisions against excess of repletion were to be enumerated, they would probably be, first, moderate or diminished appetite; second, imperfect chylification; third, predisposition to excitement in the kidnies; fourth, predisposition in the skin to perspiration; fifth, bodily exercise or labour; sixth, predisposition in the cellular membrane to the formation of fat: these provisions failing, the modes of disease, by excess of repletion, would happen, first, by a predisposition in some organ to local disease by the excitement of repletion; or, second, by a general excitement from general excess of stimulation, constituting, perhaps, one form or mode of febrile diathesis: or,

third, by a local disease from disturbance of relations incident to such febrile diathesis.

Fat and labour are among the checks of repletion; but these may be excessive, and more than in a ratio to repletion; and may, therefore, produce disease, as a state of debility or exhaustion, such as may occur on other occasions from defective nutrition.

The modes by which the formation of fat tends to prevent fever and visceral disease, from excess of repletion, are two; first, by an increase of bulk, which is like making the same quantity of blood serve two persons; and, second, by being itself a substituted disease for one in another seat: which latter mode brings us to the discussion of the other part of the doctrine of determination, denominated the cure of disease by conversion.

The full consideration of related disease will fall under a future title: at present, it is necessary to remark upon it no further than as it has been connected with the pathology of determination.

The cessation of one disease on the occurrence of another in a different seat is among the most common facts with which experience furnishes us. Thus, chronic disorder of the head may cease upon the occurrence of an abscess in the thigh, and the subject afterwards getting fat, may enjoy perfect health: thus, also, chronic disease of the eyes may cease upon the occurrence of chronic diarrhœa; fever, upon the assumption of a diseased action of the abdominal glands, and secreting surfaces; tic douloureux, upon the occurrence of a prodigious cutaneous eruption; phrenitis, upon the occur-

rence of pneumonia, insanity upon the excessive formation of fat, &c. These cases have been explained, agreeably with the theory, by attributing their phenomena to a substituted determination of blood.

It has been shown in the preceding pages, that the state of disease is a complex one, involving a change of properties, the nature of which cannot be defined, because the properties are only inferred from their effects. It has been shown also, that the determination of blood is the effect of such local disease, with which it has the relation only of a concurring cause, serving to maintain it, by supplying a material, and perhaps also to augment it, by the mechanical distention of vessels, and by the accelerated circulation which usually accompanies it. It has been shown that all the varieties and circumstances of local disease are owing to the diseased state, of which the derivation of blood is only a part, and has the force of a concurring cause; and that the continuance or cessation of a local preternatural quantity of blood, *depends* upon the continuance or cessation *of the disease which occasions it.*

To say, therefore, that the cessation of one disease upon the occurrence of another is owing to a substituted determination, is to impute a phenomenon only to one circumstance, when many are involved, and to propose that as the cause of substitution of seat, which is itself only the effect of it.

The relation of substitution in these cases is one of disease; and the derivation of blood to either

seat follows, as an effect of such disease. Thus, if disorder of the head ceases upon the spontaneous formation of an incurable wound on the back, or if insanity ceases upon the formation of anthrax, it is from a relation between these parts, by which a change is produced in the seat of the disease. The nature of this change I have discussed elsewhere *, and shall hereafter have occasion to advert to it more particularly.

The simple circumstance of derivation of blood it is always in our power to command by remedies; we may imitate anthrax by caustic issues; we may keep up a perpetual purgative action on the bowels; we may blister the surface ever so extensively; we may produce cutaneous eruptions, in any degree, by emetic tartar. All these means produce another determination; yet they will fail of curing, or substituting disease, as the natural changes do, of which they are an imitation, unless they have the same relation with the diseased state: nay, these imitations of a natural means of cure, if they agree only in the single circumstance of deriving to the parts of their application an increased quantity of fluids, without having a relation of substitution with the diseased state, so far from curing, frequently serve only to irritate the original seat of disease, and to give additional energy to its symptoms.

In gout, which furnishes perhaps the most numerous examples of substitution of seat, we can produce inflammation or vesication wherever we

* Indications which relate to the Laws of Organic Life. Chapters on Related Disease.

choose to apply the means; but if they produce only their own independent effects, they are not curative. Thus, in gout affecting the head, I have more than once in the same subject relieved this part of a disease which, being attended with delirium, difficulty of articulation, &c. threatened extreme danger, by the application of sinapisms, made with hot vinegar and mustard, to the feet: but the head was not relieved until the stimulus to the feet brought on violent gouty inflammation, which afterwards ran the course of a regular attack. Upon the supervention of gout in the feet, the symptoms of cerebral affection have immediately and wholly disappeared. But, in the same person, in the last attack of the same kind which he suffered in the head, the sinapisms, although they blistered the feet, failed to bring on gout in this situation; the calves of the legs and insides of the thighs were also blistered, but no gouty inflammation occurred in the extremities; the state of low delirium, with difficult articulation, (precisely the same symptoms as had been before repeatedly cured by bringing on gout in the feet) continued for several days, and in spite of all the means that could be employed for relieving the head, (as well with a view to a metastasis, as by bleedings from the temporal artery, cupping, leeching, purging, blistering, &c.) the affection of the brain ended in coma, effusion, and death. I have had opportunities of observing in other instances, as well as in this just related, that blisters and sinapisms, when they fail of producing metastasis of gouty disease, frequently irritate and add to the severity

of the symptoms in the original seat of the disease. When gout also has affected the chest, producing difficult breathing, sense of constriction, and suffocation at the bottom of the throat, sense of immense weight on the chest, &c. I have known these symptoms suddenly removed by sinapisms, which have excited gout in the feet, and by a blister on the chest. And on another occasion the same means failed, or rather seemed to increase the affection of the chest, which was not materially relieved until, some days afterwards, gout came on spontaneously in the feet, at a time when the vesication from the mustard plasters had ceased to be troublesome.

Thus much for the pathology of determination: the comparative merits of the treatment it suggests are next to be examined; which will be best done by an appeal to the results of experience.

The principle, that all diseases are produced by a local excess of blood, suggests a very easy plan of therapeutics.—As excess of blood is the cause of disease, there is nothing to be done but to diminish it, and to persevere in this plan as long as there is any disease; and infallibly the plan must bring the disease to a termination, either in recovery or death: the former, if it has a curative relation with the state of disease; the latter, if its relation is either to increase the disease or deviation from health, without reference to the degree in which it obtains; or if it substitutes, by modifying a natural disease, a more fatal form of artificial disease. It is well known that bleeding is by no means an universal remedy, any more

than the doctrine of determination can, with truth, be considered an adequate pathology.

The cases in which determination of blood is the most obvious are those of inflammation in its several seats. Yet in these cases the abstraction of blood is not a measure to be wholly depended upon: on the contrary, there are many others, which, in perhaps every form, are equally efficient. These measures frequently agree with the abstraction of blood in the circumstance of deriving fluids from an inflamed, to an unaffected seat: they have also another agreement with blood-letting, which is common to most other remedies, namely, that of subverting, by relations which will be hereafter traced, a condition of disease. On the other hand, remedies are sometimes curative of inflammation which actually determine blood to its seat, and at the same time quicken the circulation: this effect is exemplified by arsenic in some forms of fever, and in some inflammatory and eruptive diseases of the skin; by sulphur; by mercury, in diseases of the liver; and also by the effects of stimuli, which have sometimes, though rarely perhaps, been successfully employed in cases of this description, such as brandy, æther, laudanum, ammonia, &c.

Of the effects of blood-letting in typhus I have before spoken: and it was remarked, that many of the fatal cases of this disease would be found to be those in which blood-letting had been carried to a great extent; and that the fatal examples were rare, in which the treatment had been rested chiefly upon an efficient purgative plan.

Although blood-letting is usually and bene-

ficially employed in the treatment of inflammatory diseases, I have myself been in the habit of confiding more in other measures: and on comparing the fatal instances, treated by excessive blood-letting, with the results of my own practice in the same diseases, I cannot help considering the mere abstraction of blood inadequate to the cure of such diseases in some instances, and absolutely productive of fatal terminations in others. At least, I prefer accomplishing, to a certain extent, the same end by different means.

I have heard of persons losing a hundred ounces of blood in twenty-four or thirty hours in pneumonia, and sometimes of their having dropsy shortly afterwards. I presume, as I have met with many cases of pneumonia, and some of them very severe, that if this violent practice had been necessary, I should by this time have had reason to think so. I do not remember having had occasion to take above sixty or seventy ounces of blood from a patient in pneumonia, throughout the course of the disease: I have rarely found it necessary to take above thirty: and yet, negligent as this might appear of an important measure, an uniform success has proved to me, that copious blood-letting is not so essential as it has commonly been considered. Principally with a view of ascertaining the powers of other remedies, or how far they may stand in the place of bleeding, I have treated a case of pneumonia successfully without a single depletion of this kind. The patient took first a strong purgative of calomel, salts, senna, and jalap; a state of constant nausea, with sometimes vomiting, was

afterwards maintained by means of squill, emetic tartar, ipecacuanha, and nitre in large doses, with frequent purgatives of aloes, calomel, salts, senna, jalap, &c.; and the chest was once blistered. I have not frequently met with symptoms more severe than those of this case in the beginning; and a case never occurred to me, the progress of which was more favourable. The means just mentioned gave me almost an absolute control over the symptoms: and with those who understand their principle and employment, there are few medical agents more powerful. It is possible, by these means, in conjunction probably with a bleeding, to make a very decided impression upon an inflammatory disease; sometimes to produce syncope in a few hours after their institution: and there are few cases in which this co-operation of bleeding, blistering, purgative, nauseating, or even emetic medicines, all producing their effects upon the system at the same time, is more strikingly advantageous than in those of bronchitis, or of croup.

I have known pneumonia, in about the middle of its course, when the feet and legs became red and anasarcous, with a pulse of a hundred and thirty in a minute, treated by a diet of beef steaks and raw onions, a quart of porter, and two or three glasses of gin and water per diem, with scarcely an aperient once in three days. This patient recovered in a short time: the treatment was instituted by a very old practitioner, whose experience had been so extensive, that I know of few men in the profession who could have had so much. On mentioning this case to the late Dr. Parry, he said, he once knew

a man who fell from a scaffolding many feet from the ground, and when people expected that all his bones were broken, he got up, to their surprise, and walked away unhurt: but, Dr. Parry remarked, this does not prove that persons might fall from scaffoldings twenty or thirty feet from the ground with impunity. Few men have had a more extensive experience than the late Dr. Parry: but that of the gentleman who instituted the above stimulating plan was much greater; and he, in his turn, would consider recovery from anasarca, even though connected with visceral inflammation, by any but stimulating means, as complete an anomaly as the accidental integrity of the man's bones who fell from a scaffolding thirty feet high.

So far, however, from objecting to blood-letting in pneumonia, it appears to me that there is no case in which it is more forcibly indicated. We know that disorganization is dependent, to a great extent, upon the degree of inflammation: and where the consequences of disorganization are so generally fatal, it is an object of the first importance to diminish the violence of inflammation: and blood-letting is proved, both on principle and by medical experience, to be at least powerfully auxiliary to this end. I have commonly found that two bleedings of twenty ounces each within the first thirty hours, with perhaps a bleeding of eight ounces on the third day, and two or three smaller ones of five or six ounces in the course of the disease, has done all that was to be expected from blood-letting. I have had reason to think, that repeated copious bleeding has kept up or

increased the accelerated action of the heart; and the fatal cases which have come to my knowledge have been chiefly those in which blood-letting has been solely confided in, and therefore carried to a great extent, without being aided by means which are certainly equally powerful in subverting inflammatory action, such as nauseating, emetic, and purgative medicines, together with doses of nitre from a scruple to half a dram.

During the winter of 1820 and 1821 bronchitis prevailed as an epidemic; and many of the cases, after the first inflammatory violence of the symptoms was subdued by bleedings, blisterings, and all the artillery of the antiphlogistic plan, ran a long course, which threatened to end in consumption. The chronic symptoms were cough, short breathing, pain, or cough, or both, on inspiration, moist and sometimes clean tongue, pulse averaging from ninety to a hundred or a hundred and ten, flushing of the face, heat of skin, and buffed blood. In some of these cases, in which these symptoms did not yield to the usual remedies, and their fatal termination appeared almost inevitable, I have given two grains of calomel, with James's powder, three times a day. I had not occasion to resort to this practice in more than four or five cases, and they all recovered. The calomel quickened the pulse a little before the gums were affected, but after a slight ptyalism was produced, the pulse commonly subsided to seventy; the patients lost their other symptoms, and were regularly convalescent from that period. I have succeeded also by suddenly mercurializing

the system, after bleeding, blistering, and a purgative have been premised, in bronchitis, which, from having been previously neglected, has, in an advanced stage, threatened suffocation every hour. In order to make the greater impression on this state of disease, I have combined emetic tartar with the mercury, as by giving two grains of emetic tartar, and ten of calomel, every six or eight hours. These medicines, in this combination, are capable of producing their separate effects. Of the effects of mercury in consumption, we have had many contradictory accounts. The reports of Dr. Rush of the efficacy of this treatment, gave rise with some to sanguine expectations that a remedy was at length discovered for this medical opprobrium. I have tried mercury to the extent of producing salivation, which in general in this disease is not easily done, in many cases. I have known it suspend all the symptoms of tubercular phthisis; the patient has not coughed once during the ptyalism. Whether the remedy would have succeeded if the ptyalism, which was severe, had been longer maintained, I cannot determine: but although for about ten days or a fortnight the symptoms were suspended, they recurred as the mercurial affection abated, and the patient died of consumption after a long course of the disease. This is not the only instance in my own practice in which mercury has produced a suspension of the symptoms of this disease more or less complete: the recovery has been so considerable, that patients were considered well by their friends:

but I never knew a permanent recovery from phthisis by mercury, or any other means, where the tubercular form of this disease was characterized. I have, on the other hand, known cases of ulceration, following acute inflammation of the lungs, which has become chronic, and continued many weeks with quick pulse, colliquative sweats, pain on inspiration, buffed blood, and purulent expectoration, which have recovered perfectly after salivation. Where there is not, as in tubercular phthisis, a mechanical cause of irritation, or where the local disease of the lungs is that merely of ulceration which is indisposed to heal, I believe that the change of action produced by mercury may improve this disposition, subverting, at the same time, the febrile tendency: and the general determination produced by mercury to the glandular system will, in either form of phthisis, give at least relief of an hydraulic kind, to the seat of the disease. But, as before remarked, it is sometimes extremely difficult (I have once found it impossible) in these, as well as in some other forms of organic disease, to make mercury produce its usual effects. It will quicken the circulation: but the local determination, which is in general connected with every preternatural diathesis, will oftentimes be to the seat of the disease, rather than to the glandular system, on which mercury at other times appears to exert so specific an operation. The symptoms of the disease, under such mercurial influence, are in these cases rather increased: and the vascular action excited is un-

favourable to the local disease, or rather concurs with the particular effects of mercury on its seat, to hasten its progress.

It appears sufficiently evident, that mere ulceration of the lungs is not necessarily fatal. It is not uncommon for persons to recover who have received a wound by a small sword in the lungs, or by a bullet which has gone quite through one lung, and perhaps has been cut out from the back. The fact is more strikingly shown by the cases of recovery from empyema. There are also specimens in museums of the entire destruction of one lung by suppurative disease; in one of those which I have seen, in which there were scarcely any remains of the right lung, the subject lived nearly twenty years after the symptoms of active disease had ceased. The fatality of ulcerative disease of the lungs appears to be owing to associated circumstances of disease, such as a general inflammatory diathesis, manifested by hectic fever and buffed blood. To what extent mercury, carried so far as to produce salivation, which effect should not be suffered to decline rapidly, might succeed in the design of subverting the disposition of disease which is allied with suppurative disease of the lungs, I do not possess a sufficient number of facts to determine. But, from the success of this remedy employed with this design in a few cases, as well as from collateral experience of the powers of mercury, when carried to the extent of salivating, in other cases of obstinate inflammatory disease, I should anticipate that its advantages in the cases in question might be very considerable.

Although bleeding ought to be, on *à priori* views, and generally is, productive of vascular relief, my experience of this remedy in any form of phthisis (which experience has not been very extensive) is decidedly against it. I never knew a patient recover from any form of phthisis in which bleeding was trusted to as the principal remedy, and therefore practised to a great extent: on the contrary, I have seen cases terminate more rapidly in death, than I think they would have done if left entirely to nature, in which this practice has been pursued violently.

Setons, issues, and perpetual blisters, have also generally appeared rather prejudicial in phthisis, than otherwise. In one instance the progress of tubercular phthisis appeared to be retarded by a small perpetual blister on the side: but it was in this case kept open more than a year without trouble; it discharged regularly, and was not productive of the least irritation. During this period tubercular phthisis was in this patient sufficiently indicated by symptoms: he had occasional night sweats, some loss of flesh, a pulse not exceeding ninety, and he sometimes coughed up pieces of phosphate of lime: but the disease did not make progress, and he attended to his business, which was not laborious, with but little interruption. He became tired of doing tolerably well: he was not satisfied with not getting worse, but was desirous of getting better; and therefore obtained other advice: the result of which was, that the blister was dried up; and instead of a palliative treatment of the least irritating kind, he took a

steel mixture; and in a few weeks afterwards his pulse, instead of averaging from eighty-four to ninety, averaged from a hundred and twenty to a hundred and thirty. His feet became œdematous, the local symptoms of the chest were greatly increased, and he died in less than two months from the change of treatment.

I have known another patient who had tubercle of the lungs, and occasionally coughed up phosphate of lime, whose symptoms were suspended, when they seemed inevitably approaching to a fatal termination, by a slight ptyalism; so that he resumed his occupation for two or three months without interruption. An exacerbation of fever with increased local symptoms then occurred; and these gave place to melœna, or were suspended in great measure while this disease prevailed, upon the cessation of which the liver became enlarged and indurated, he had ascites, and died.

The means of counter-irritation, or the establishment of local drains on the chest or side, appear useful when there is local irritation of the lungs, without much constitutional irritability. Of these means I have thought the application of adhesive plasters, containing from five to ten grains of emetic tartar, for two or three days, the most beneficial. If these means act favourably, they are at best but auxiliaries in the treatment of phthisis: but more frequently I have had reason to think that they have produced a constitutional irritation which was by no means compensated by a temporary and perhaps doubtful local relief. In

tubercular phthisis, it is, I believe, best to treat symptoms simply with a view to their mitigation; it is a state of system which it is dangerous to excite: and the only excuse for the employment of powerful measures is the certainty of a speedy termination of the disease in death, without them; or, if there is but one chance of cure in a hundred, to give the patient that chance.

The remarks which have been hazarded on the auxiliary powers of medicines to blood-letting, or rather on their superiority in some instances, hold good in most inflammatory diseases. In acute rheumatism it is customary to depend almost entirely upon blood-letting. I have heard of cases in which death was supposed to have happened from effusion into the pericardium, in which this practice has been pursued unsparingly. I never myself met with a fatal case of rheumatism, although I have met with many severe ones. In some instances I have taken very little blood, perhaps not twenty ounces in the course of the disease, from observing that the symptoms were much more decidedly influenced by purgatives with calomel, elaterium, aloes, salts, senna, &c. together with full nauseating doses of emetic tartar and ipecacuanha, which sometimes produced vomiting, &c. The quantity of blood taken must of course depend upon the particular indication of the symptoms, as of the pulse, &c.: but the benefits of purging have appeared to me more decided than those of blood-letting; more particularly in the middle or chronic stages of the disease, in which blood, taken at the end of six

weeks, will be as buffed and cupped as that taken during the first three days. Recovery from the chronic stage, and where the blood has been so cupped, has been promoted by a cautious treatment by improved diet, and even bark, together with regular, but not violent purging; and if ground is lost by the trial of this plan, it is easily recovered by a more active purgative, followed, perhaps, by emetic tartar, ipecacuanha, nitre, &c.

I have frequently known the heart affected by rheumatism, and, in some cases, I have bled, though never copiously.—Not that bleeding may not be highly proper, but that I think it an auxiliary only to measures which are more powerful.

A patient had acute rheumatism severely (pulse 130, &c.); she was a large woman, about forty years of age; she had been bled (though with difficulty), purged, &c. About the fifth day she had violent pain in the left side, with sense of suffocation, and intermittent pulse, which could scarcely be counted. Owing to her being exceedingly fat and the veins very small, I could get no blood. I put a blister on her side, gave her a large dose of calomel, aloes, salts, senna, and jalap, so as to produce ten or twelve stools; and afterwards doses of emetic tartar; and in sixteen hours her pulse was regular and reduced to 110, the pain in the situation of the heart, sense of suffocation, &c. entirely removed, and the woman had a favourable convalescence.

A man, who had been freely bled, &c. in the commencement of acute rheumatism, about the

second week had sense of weight on the chest, with frequent faintings; at least, he appeared to those around him in a state of syncope; his face was at these times pale, his lips blue, and his pulse weak, hurried, and intermittent. In this case, or rather in the treatment of this stage of it, I trusted wholly to the means of revulsion before mentioned, together with blisters. This man recovered. Three years afterwards he had a similar attack of rheumatism of about the same severity, which ran a more favourable course than the former one, entirely owing to the more liberal use of effectual purgatives; as calomel, elaterium, aloes, camboge, senna, jalap, &c.

A gentleman had chronic rheumatism, chiefly affecting his knees and shoulders. He went out on a cold, damp day: in the evening he had rigors; the rheumatic pains left the extremities, and he was taken with something like syncope, sense of constriction at the bottom of the throat, of weight on the chest, with a fluttering irregular pulse of about a hundred and sixty in a minute. I opened a vein in the arm, but could not get more than four or five ounces of blood, which was buffed. I gave him a full dose of calomel, salts, senna, and jalap, which produced eight or ten stools in as many hours, covered his chest with a blister, &c. The next day he was able to lie flat in his bed, and his pulse was more regular and slower, though still quick and intermittent. He took repeated doses of squill and calomel, with salts, senna, &c. every morning. The squill sometimes produced vomiting, and in about four days

his mouth became affected by the calomel; his pulse almost immediately afterwards came down to sixty, was perfectly regular, and all the other symptoms ceased. He was confined a few days to the house by the soreness of his gums and tongue; this affection subsiding, he recovered rapidly.

I have found chronic rheumatism in other instances of considerable duration yield, after bleedings had been premised, to alterative doses of calomel with sarsaparilla. The gums have commonly been a little affected before the symptoms ceased, and the alvine evacuations have been copious, both during the course and after it, without any additional purgative. I have tried colchicum in a few cases, but it has never appeared to me to be any thing more than an uncertain purgative.

I could swell this list of cases considerably; but all those which have occurred to me agree in the testimony of those mentioned. It is possible to produce syncope, in many inflammatory diseases, only by the use of combined purgatives, and emetic tartar; and the reduced action thus produced is more permanent than that which succeeds to the syncope produced by blood-letting. The latter, however, is a highly important measure: my objections are not to its judicious employment, but to an entire confidence in it, together with a neglect of other means, which are not less powerful, if their employment is understood. This limitation may with truth be extended to most, or perhaps all, other inflammatory diseases.

In peritonitis, I have trusted also much more to purgatives than to bleeding, and I have no reason

to regret this confidence; for out of many cases, under every variety of circumstances, I never met with one which terminated otherwise than favourably. It has appeared to me the first object in inflammations of the bowels, whichever coat may be the seat of it, (and on this point our diagnosis is imperfect) to overcome the constipation with which such inflammatory disease is commonly, or frequently attended. This design is chiefly founded upon the experience that, when this effect is accomplished, the cure is in our own hands. The theoretical design of this practice is less to evacuate the bowels, on the supposition that their contents may be instrumental to the disease, than to subvert the state of disease in the bowels, by the employment of measures which act upon its seat. Among other modes of the operation of purgatives in inflammation of the bowels, perhaps the secretion which these means excite throughout the intestinal canal, is not the least important. We know that secretion is an ending of inflammation, and that it is frequently the spontaneous mode of relief to the vessels of an inflamed seat.

The use of purgatives in enteritis has been objected to, on the ground that they act by stimulation; and that stimuli, applied to its seat, must increase inflammation. The effect, however, is otherwise; and the testimony of experience must on this, as on other occasions, supersede all *a priori* reasoning. But, as a matter of reasoning, perhaps the conclusion against purgatives on this ground is not legitimately inferred. It does not follow, that an agent which is related with a secreting

function, so as to increase it, should also be so related with inflammation (which frequently suspends secretion,) as to augment its intensity. On the contrary, in the way of reasoning, it would appear, that if secretion is suspended by inflammation, that which restores secretion must diminish inflammation. Setting reasoning, for the present, aside, I suspect, that in the cases in which purgatives have been supposed to increase intestinal inflammation, it is because these means were inadequately employed.

The constipation of intestinal inflammation is generally attended with vomiting, and almost every thing is rejected which is taken into the stomach. It is common, therefore, to regard purgatives almost as useless; to project the reduction of inflammation by bleedings, and afterwards to give purgatives. The operation of the purgative has appeared to me of the first importance: and, although I have generally bled in the beginning of the treatment, I have never rested until I have got the bowels opened; and I never failed in this attempt, unless the obstruction was of an organic or mechanical kind. For this purpose, I have given at first six or eight grains of calomel, followed in an hour by a black draught, with half a dram of jalap. If this was rejected by vomiting, it has been repeated in an hour or two; and if again rejected, again repeated, alternating perhaps these doses with other forms of purgatives; as with castor oil, in two and three ounce doses, with pills, containing calomel, elaterium, scammony, camboge, &c. together with frequent soap glysters.

This is often a sort of contest between the practitioner and the stomach; and the point of emulation is, whether the doctor will first be tired of dosing, or the stomach of rejecting: I have always had the good luck, or the perseverance, to be the victor on these occasions. If a considerable part of the medicine is rejected, some will be retained; and the quantity retained accumulates in proportion as it is repeated, and at last, with the aid of injections, produces stools, at first scanty, afterwards more copious; and then the vomiting ceases, the tension of the abdomen is relieved, and the soreness diminished. This effect gained, I have never found bleeding necessary afterwards. A little blue pill and aloes at bed-time, with a draught of salts, senna, and perhaps jalap, every morning, will be a preventive of inflammation in the progress of the case; during which the bowels recover their disposition to healthy action.

Among other instances of intestinal inflammation, I have met with many of puerperal peritonitis. They have invariably done well under adequate purging: bleeding has, in some instances, been premised, but never carried to a great extent; and of this disease, as of some before mentioned, I may say, the greatest number of fatal instances which have come to my knowledge have been those in which the treatment has been rested upon bleeding, to the neglect of other measures. Blisters on the abdomen are useful auxiliaries, if the irritability of the system will permit their employment. Leeches may also be beneficial; but both these means are, I believe, chiefly useful, inasmuch as they furnish

an excuse for covering the abdomen with a large bread and water poultice.

Although I have never myself employed spirit of turpentine in puerperal peritonitis, I have had opportunities of witnessing the effects of this practice; and the results, so far as they have been known to me, have been favourable. The mode in which it has been given has generally been in doses of two drams or half an ounce of the turpentine, with an equal quantity of castor oil; and either of these doses has been repeated, perhaps, in six hours. The mode of operation of this remedy has appeared to me to be the common one of purgatives; of which class of medicines it is one variety.

Although the constipation attendant upon intestinal inflammation is in general overcome by the means first mentioned, I have met with cases in which these means have failed; the disease has, in such, appeared rapidly approaching to a fatal catastrophe. I have, in these cases, after the unequivocal failure of other means, given ten grains of calomel every six or eight hours; and in the few instances in which I have had occasion to employ this *dernier ressort*, it has succeeded: copious stools have quickly followed a sudden salivation; and a favourable convalescence has afterwards been maintained by purgatives of the weaker sort. I know of no case in which this remedy, pushed to such an extent, is more indicated by principle than in this: the general determination to the glandular system before spoken of, is one among other modes of subverting inflammation in

a seat of disease, and the importance of resorting to it is great, where the continuance of inflammation may be fatal by its relation with the function of the structures which it invades, or by the prejudice which, in other instances, may be suffered from disorganization. On this latter ground, the practice has been adopted with a success to which my own experience bears concurrent testimony in iritis, the progress of which I have found immediately arrested as soon as the saliva began to flow. I have also, in children, known constipations which have resisted all previous means give way when the mouth has become affected by calomel.

It sometimes happens that peritonitis is attended with diarrhœa, instead of constipation. The purgative practice is neither less essential, nor less successful, in this than in the other form: the difference is, that purging is in these cases more easily maintained, and by less violent means.

In phrenitis, arising from local injury of the head, or unconnected with fever of any peculiar type, the advantages of blood-letting, or rather its superiority, to all other means, are very conspicuous. Purgatives and nauseating doses of emetic tartar are very important auxiliaries; but blood-letting has appeared to have a more decided control over the symptoms than any other measures. In these instances, blood-letting is, of course, indispensable; and I have known every exacerbation in the progress of such a case denoted by increased restlessness or delirium, together with an increased frequency of the pulse, of perhaps ten, fifteen, or twenty beats in a minute, regularly

subside, after a bleeding of six or eight ounces; and perhaps a dozen such exacerbations, with an uniform result from the employment of this remedy, might have occurred in one severe case. The necessity of blood-letting will, however, be less frequent in general, if an efficient purgative and nauseating plan make a part of the treatment. As a topical measure, drenching the head by the constant application of cloths dipped in cold water has appeared the most beneficial in the beginning, more especially if there is much preternatural heat of the scalp, or flushing of the face; and the advantages of blisters on the scalp have been the least equivocal, when the powers of re-action have been diminished by a short course of depletion.

I have reason to think the control of blood-letting over the state of puerperal mania less marked. I have treated cases of this description wholly by purgatives; or, perhaps, with the addition of a few leeches, which, by diminishing the tension of the vessels of the head, often afford greater relief than more copious bleeding from the arm. I have known puerperal mania occur in connexion with flooding at the time of delivery; and I believe there are in medicine abundant facts which prove that determination to the head is a frequent consequence of a great loss of blood. Rather an extreme case of the occurrence of symptoms, which are usually in connexion with determination to the head, in consequence of the loss of blood, is furnished in the following example: A woman was delivered suddenly and unexpectedly, whilst standing in her room, at a distance from the bed;

the child fell on the floor, but the funis was not broken; there came on instantly a copious flooding. I found her, as soon as I arrived, insensible, and the floor was deluged with blood: she had no pulse at either wrist, &c. Twelve hours afterwards she had a pulse of a hundred and twenty in a minute, bounding of the carotids, dilated pupils, and complete insensibility. She lay in this state more than a week. I purged her every day; her pulse was always reduced after the action of the purgative, and regularly increased during any long interval of its operation. This woman was a catholic; and at the end of a week, as it was expected she was to die, she received extreme unction, and every thing was made ready for this event. I continued the purgative of calomel every night, and salts, senna, and jalap, the following morning. On the ninth day she recognized her husband and children, spoke to them, &c.; and, as she was better, I thought I would omit the purgative: she had no stool for about twenty-four hours, and then relapsed into her former state of insensibility. I gave her an active purgative, and she recovered her faculties and speech, in as great a degree as before the omission of the purgative, as soon as it had operated. Her convalescence from this time was regular, and during the whole period of it, a slight purgative, frequently repeated, together with a gradual repletion, kept her free from any considerable degree of affection of the head.

In ordinary insanity the experience of a general practitioner is, of course, comparatively very limited. In a few cases in which I have known

blood-letting carried to a great extent, not the smallest advantage has appeared to result from it. In incipient insanity, sufficiently characterised, I have known the disease cease totally in a few days under a violent plan of vomiting and purging, as by three or four grains of emetic tartar, and six of calomel, every six or eight hours, together with salts, senna, and jalap, at intervals. A young woman became gradually insane, which was supposed to be in part owing to love, and in part owing to religion. She was under treatment for about a fortnight, consisting chiefly of bleeding, which was very copious: she took, in the course of this time, several doses of calomel at bed-time, with a purgative the next morning: during this period there was no abatement of symptoms. The calomel then affected her mouth, and a violent salivation ensued, with ulcerated tongue, &c. During this salivation she was perfectly sane in her intellect; the catamenia appeared about a week after its commencement, and she remained well about a fortnight; when the mercurial affection had ceased, and the insanity returned. The case became hopeless, and she was sent to St. Luke's, little more than a living skeleton: she remained thin and mad; and at the end of eight months was reported incurable; she then became enormously fat, and before the twelvemonth of her residence at St. Luke's was expired, she was dismissed well. She continued fat, and sound in her mind, for many months; and gradually became again thin and mad; and has remained thin, and mad, ever since. The cure of insanity, by obesity,

is remarked as a common circumstance: it may be cured also by an abscess, by a carbuncle, or by pregnancy; facts which, if duly considered, are not very complimentary to human genius.

It appears that the merits of blood-letting, as a remedy in acute disease, are, in some forms of such disease, very obvious; in others, its efficacy is inferior to that of some other means: that in the former, purgatives, &c., may be highly advantageous, as auxiliary to blood-letting; and that blood-letting in the latter may have an auxiliary importance with a treatment chiefly rested on purgatives. In some chronic diseases, in which determination of blood is even a conspicuous symptom, the propriety and extent of blood-letting are of, perhaps, more difficult discrimination.

Apoplexy is supposed to be produced by determination to the head. We have seen in our examination of this pathology, that local determination of blood is always a symptom, or an effect of previous disease. The curative dependence of blood-letting, as well as of all other means, is upon the relation which this remedy has with the state of disease. Bleeding has one direct curative relation with the state of disease; namely, that as disease is maintained by assimilation from blood, and as, in disease, the life of its seat is, in general, under preternatural excitement, and therefore derives blood in a greater quantity, which has the final effect of supporting the more rapid consumption of life; so the reduction of the quantity of blood tends to reduce the energy of disease, by diminishing the supporter of this state. But

it may have indirect relations with disease, which may counterbalance the advantages derived from this direct one; namely, a worse form of disease, or one producing other phenomena may be substituted; and these effects might ensue from the relation of blood-letting with a certain state of properties in the department of life; or from a certain state of the structures: these relations of blood-letting with the state of disease are exemplified abundantly by the results of the practice. The remedy, therefore, though tending to diminish the supporter of disease, having complex relations which we have traced in part, and shall trace more fully hereafter, may change the form of disease; and from this change of form the tendency to derivation of blood might be increased, or sensible effects, constituting additional circumstances of disease, may be denoted by symptoms observed in the structures.

We know from experience that blood-letting is not curative even of that symptom of disease which consists of increased determination, unless it is curative of the disease. From the direct relation of blood with disease, namely, that of a supporter, the reduction of this fluid can never do more than reduce the energy of disease; if it cures disease, it is by other relations, by which the state of disease is changed; these relations are of an indirect kind: and whether blood-letting has a curative relation with disease, or one which is productive of another, and perhaps a more unfavourable state of disease, can be judged of only by our experience of the employment of blood-

letting, in connexion with certain forms of disease. If blood-letting has no tendency to subvert or cure the state of disease, this state will continue to produce the symptom of determination as long as the state itself continues; and to whatever extent depletion may be carried, as long as the same disease occupies the same seat, this seat will derive a disproportionate quantity of blood. The curative efficacy, then, of blood-letting depends upon its having a relation of this kind with the state of disease: it has a direct tendency to diminish the energy of disease, by the direct relation subsisting between life and blood; and it may contribute also indirectly to the same purpose, by diminishing vascular distention, which is known to increase disease, and, related with predisposition, may sometimes excite it, or modify its form. This influence of blood-letting on disease is indirect; it is, through the medium of hydraulic or mechanical relations; and by a relation of the same kind, the tendency to secondary disease, affecting the structure of the vascular system, may be restrained, or its spontaneous effects prevented. The test of the curative efficacy of blood-letting is experience; and excepting the credit which the remedy obtains in quality of a means, by which that fluid is reduced which supports disease, or may augment it, or impair the structures by mechanical agency, blood-letting has, on principle, an efficacy only which is common to all other remedies; and is liable to produce the results of other remedies in connexion with disease, namely, a substituted form of disease, ending in recovery, or in death.

I am disposed to think that blood-letting, as a remedy for apoplexy, is in some, though not in all, cases overrated. As bleeding is almost invariably resorted to in apoplexy, our experience of its efficacy is, on this account, defective; for a perfect experience of a remedy is obtained only by a comparison of the results of its employment, and of its neglect, in the same cases. As this is a question which is to be tried by experience, the mention of a few cases may, perhaps, best serve to illustrate the points on which our discrimination is to be exercised.

A gentleman between sixty and seventy years of age had vertigo, and a quick pulse. He was bled repeatedly from the arm, and cupped, and lived on gruel and vegetables. During this treatment, and after considerable depletion, about a fortnight from its commencement, he had a copious bleeding from the nose, and lost, at this time, at least a quart of blood. The vertigo continuing, he lost fourteen ounces of blood by cupping; a few days afterwards leeches were also applied to the head, and this system of depletion was continued about a fortnight longer, without any change of plan, when, with perhaps a hundred ounces of blood less in his system than was formerly compatible with health, he died of apoplexy. It is needless to remark that bleeding was inadequate to cure the state of disease in this case, or to avert the effects of a tendency. Will it be said that, although the natural quantity of blood was reduced, the relative quantity, to the state of the system, was still in excess? This is a most per-

nicious sophism: if the quantity of blood is less, and the irritability greater, than natural, it is surely more correct to impute the effects of this relation rather to excess of irritability than to excess of blood.

A gentleman of a full habit, between fifty and sixty years of age, had apoplexy after dinner; he was bled, and recovered his speech in a few hours, but had lost the use of one side: his pulse was a hundred and twenty. He was bled the same day from the arm, and fifty ounces of blood were taken from the temporal artery: he was bled again the next day, and cupped in the evening. During the whole of this time, from the recovery of his speech, he recognised his friends, and talked with very little incoherence. The bleeding had chiefly for its object the reduction of the pulse: in this intention it failed totally. He lost about a hundred and thirty ounces of blood in the first two days, and on the third he died. These patients I saw only by accident, or from locality, once or twice.

A man about fifty years of age, of a full habit, was taken about five o'clock in the morning with vertigo and vomiting. I saw him about eleven o'clock: his pulse was full and slow, his face flushed, and he complained greatly of his head. I took about twenty-five ounces of blood from his arm. Before I left the room, immediately after the bleeding, another attack of vomiting came on, during which his face became almost black; he fell back on the pillow as if shot, the sphincters of the anus and bladder gave way, his jaw dropped,

and he was dead in less than a minute. On this case some may say the bleeding was inadequate; others, that the impression, or change occasioned by it in the state of the vascular system of the brain, disposed the vessels to rupture, under an impulse of blood, which they would otherwise have sustained.

A thin woman, with a pale face, slow, soft pulse, about forty years of age, had a slight attack of apoplexy, which left a little distortion of the mouth. She was afterwards bled and cupped, by way of prevention; and one of the wounds made by the scarificator bled copiously many hours: it appeared as if the blood had unusually weak powers of coagulation. I have known uterine hemorrhage habitual in a person who, during her pregnancy, was accustomed to eat with a spoon enormous quantities of salt, and whose blood also scarcely coagulated. Whether a corresponding weakness in the tonic power of the arteries disposed to hemorrhage by rupture in one case, and by want of contractile power in the other, we have in part discussed, and other facts may determine; in the meantime, the hemorrhage may, perhaps, be supposed to be more immediately dependent upon the defect of coagulating power in the blood. In addition to these preventive means of depletion, a seton was, in this case of apoplexy, put in the back of the neck, and the woman was enjoined a low diet. During the adoption of this plan, she had an attack of apoplexy, and died immediately. It was presumed that some very considerable vessel was ruptured, although the precise vessel could not, in a hasty

examination, be ascertained. More than a pound of blood was extravasated, and the structure of the brain was so broken down that it was difficult to make out any distinction of parts.

A gentleman had an attack of apoplexy followed by partial paralysis of one side. He complained one morning of his head, and lost blood from the arm: on the evening of the same day he had apoplexy, for which he was bled from the temporal artery: he recovered from this attack, but died of apoplexy a few months afterwards, under the depleting treatment and regimen.

A young woman, whose circumstances had subjected her to much fatigue of body, and mental distress, complained of pain in the head and vertigo: she was bled, and an emetic was given her: during the action of the emetic, or immediately after it, she became apoplectic. My acquaintance with the case did not commence until two days after the attack, during which period she had been insensible, and she had been bled almost every five or six hours: on the third day she died. Some blood had been effused, though the quantity was trifling, about the basis of the brain, and the ventricles contained about three ounces of serum. This case, in connexion with two beforementioned, in which apoplexy, preceded by vertigo, &c., occurred during the action of vomiting, indicates that there is some hazard in the employment of emetics in such cases. On the other hand, I have known insensibility, with dilated pupils, &c., in children, cease as soon as full doses of emetic tartar, repeated every twenty minutes, produced

vomiting: in the cases alluded to, bleeding from the temporal artery had been premised without any apparent advantage.

A woman had an attack resembling apoplexy in some circumstances, but not in all; that is, during the fit she appeared to recognise persons, and made efforts to speak: her pulse was about ninety. She was bled, purged, &c. This state, with little deviation, lasted three weeks, during which time no paralysis of the limbs occurred; the loss of muscular power appeared to interest exclusively the organs of articulation*, and she frequently showed signs of intelligence. During this long period she was bled copiously and repeatedly. The blood from the arm never exhibited any appearance of fibrine; the blood from the temporal artery exhibited always this appearance, although the bleedings were in the order, from the arm in the evening, from the temporal artery the next morning, from the arm in the evening of the same day, and from the temporal artery on the following, or second day afterwards. When every other treatment had been pushed as far as possible, she was put imperfectly under the influence of mercury, by mercurial friction: the action of the mercury on the constitution was but faintly marked, and a trifling benefit was thought to be gained from it: she however died at the end of three weeks from the first attack. Small masses of

* I once attended a woman in apoplexy, from which she did not recover in less than ten days or a fortnight. This disease was followed by incurable paralysis, exclusively in the distribution of the glosso-pharyngeal nerve.

coagulum, and patches of blood, were deposited in the brain, and on its surface, in forty or fifty places; and the ventricles contained about five ounces of serum.

I could enumerate many more cases, in which blood-letting has been proved to be inadequate to the prevention or cure of apoplexy: and from some others which have occurred to me, I have been sometimes inclined to suspect, that if the state of disease is one which tends to apoplexy, this result will occur, in perhaps about the same time, either with bleeding or without it.

I attended a man who was an habitual drunkard in three or four fits, which appeared to be apoplectic: his breathing was stertorous, pupils dilated, and insensibility, without convulsions, complete. In some of these attacks I bled him copiously; in others, the bleeding was insignificant, and too trifling to produce any effect: he recovered from each attack in about the same time; and the bleeding on the former occasions, and the almost total neglect of it on the latter, appeared to have no importance with respect to the disease.

A gentleman, about seventy years of age, fell from his chair in a fit. He was insensible, with strabismus: one side of the mouth was considerably distorted, his pulse was quick and intermittent, and his extremities cold. I opened a vein in the arm, but could not obtain more than three or four ounces of blood. I contrived to give him a purgative, put his feet into hot water, and afterwards applied mustard plasters to them. He remained insensible about six hours, and recovered

as soon as a large dose of calomel and jalap had operated. No paralysis followed in this case.

A man of a full habit, about seventy years of age, had an attack of apoplexy, as completely characterised as it could be by symptoms. He had no perceptible vein in the arm, and I took about sixteen ounces of blood from the temporal artery, gave him calomel, jalap, &c. He recovered. A few months afterwards he had another attack, resembling the former one: I took eight ounces of blood from the temporal artery, and he recovered as favourably as before, in about two days. A twelvemonth afterwards he had another attack, which appeared likely to be a fatal one: he was totally insensible, with dilated pupils, one side of the mouth drawn considerably towards the ear, saliva flowing from the mouth, &c. I opened a branch of the temporal artery, but it did not bleed freely: he did not in this attack lose an ounce of blood; and although it was, apparently, more alarming than either of the former ones, he recovered from it not less favourably.

A gentleman who had previously had some supposed attacks of apoplexy, for which he was bled, and afterwards recovered, had a fit of a similar kind at a distance from home: some delay occurred in procuring medical assistance, and before this could be obtained the gentleman had recovered.

Blood-letting, as a measure of prevention where the tendency to apoplexy prevails, appears liable to the following objections: 1st, that the loss of blood disposes frequently to a more rapid formation of this fluid, of which there is, I believe, of

course with many exceptions, abundant testimony: 2d, frequent and copious bleeding often produces determination to the head, or increases it, where it before existed: 3d, there are states of the subjects, disposed to apoplexy, in which any change, operating as a cause of excitation, may induce the effect of the tendency; which seems to be resisted only so long as some remains of an habitual balance of functions continue to counteract it: 4th, frequent bleeding increases the proportion of serum in the blood, and, under ordinary states of this fluid, tends to weaken the coagulation of the crassamentum; the muscular or tonic power of the arteries, as is indicated by some cases, bears a proportion to the coagulating power of the blood; and the diminution of this power may concur in exchanging mere determination for sanguineous apoplexy. The force of these objections is by no means insuperable: they may, however, be borne in mind, and their opposition is rather to superfluous or unnecessary bleedings than to those in which the propriety of the measure is fully confirmed by the hazard of a much worse alternative.

Blood-letting, in apoplexy, as a means of cure, is indicated both by principle and experience to a certain extent. But, it is suggested by many results, that although bleeding to a certain extent may relieve the vessels, and tend to produce a more equal circulation, an excessive loss of blood is commonly itself productive of an irregular circulation, and will therefore tend to increase it where it exists, and perhaps will also concur with the tendency of the disease to a rupture of vessels,

by diminishing the power which these structures possess, of resisting the impulse of the blood.

As we know that apoplexy is sometimes produced by disease of the heart, or vessels immediately proceeding from it, impeding the passage of blood through the right side of this organ, and therefore occasioning venous congestion of the brain, so it appears probable, from the consideration of other examples, that the heart, which sympathises so readily with the stomach, may, by a disordered function, alone produce a disorder of the head of any degree, from mere sense of fulness, to vertigo and apoplexy. We have seen how accelerated circulation alone may in some cases excite disorder of the brain, where this viscus is predisposed to excitement from this cause: but the affection of the heart, producing venous congestion of the head, is of a different kind: it does not consist of accelerated action, but if, without organic disease, it impedes the return of blood through its cavities, the effect of its sympathy in such cases with the state of the stomach must be a spasmodic contraction of its fibres, diminishing or constricting the auriculo-ventricular opening on the right side, when the head is the seat of consequent congestion, and on the left, when a similar congestion occurs from the same cause, as it sometimes does, in the lungs. Cases are not uncommon of persons, whose health is habitually good, and in whom there is no reason to suspect organic disease of the heart, becoming almost black in the face, perhaps after dinner, and falling off their chairs in a fit of apoplexy. The deep

colour of the complexion, together with the absence of accelerated action of the heart, prove, as far as may be done without dissection, that the congestion in these cases arises from venous obstruction. As we have reason to think that the veins are not liable to a contraction which would impede the transmission of their blood, and that they are almost passive in the phenomena of the circulation, so it seems most probable that the seat of obstruction should, in these instances, be in the heart itself, the ready sympathy of which with the stomach is proved by many facts.

I am acquainted with a gentleman, about forty years of age, who in general enjoys the most perfect state of health; certainly he has no symptom of organic disease of the heart. Yet in the course of every year he has perhaps one or two attacks of the following kind. After a dinner of no remarkable excess, his face, which is usually pale, has on a sudden become livid, his features appear swollen, he loses his recollection for a few minutes, and, if standing, is obliged to seek immediate support against the first object that presents itself. This attack is accompanied by a sense of suffocation, or oppression of the chest, and his pulse at the time does not much exceed seventy in a minute. In about a quarter of an hour the confusion of mind ceases, his face resumes its natural complexion, the oppression of the chest has also ceased, and he enjoys the most perfect health for many months: he never lost blood, or took medicine, during any of these attacks, or afterwards, and his health is so good, that he has not taken

ten doses of medicine in the last twenty-five years.

I have sometimes suspected that apoplexy was occasioned by mere congestion of the vessels of the brain, without extravasation; that insensibility is then produced by the pressure of a preternatural volume of blood contained in its vessels. This state of apoplexy, if it could be distinguished, would appear one of less danger than that occasioned by extravasation. The proof of apoplexy from this cause would be furnished by the results of a careful examination of the brain of a person who died soon after an attack, in which this state of the brain was supposed to have obtained. Any other proof seems defective: the diagnosis is not furnished by the absence, or succession of consequent paralysis; for it has been known that those who have had apoplexy, with rupture of a blood vessel, have got up and walked away when the fit was over: and on examination after death, the number of apoplectic cells have been found to correspond with the number of fits, some of which were not followed by paralysis.

The possible modes by which the vessels of the brain (and these modes are not peculiar to this viscus) might become congested, appear to be the following: 1. A preternatural derivation of blood by the capillaries of the arteries. 2. An accelerated action of the heart, by which blood may be urged into the arteries faster than it is returned with facility by the veins. 3. Deficient action of the arterial capillaries; or of those of the returning systems of the circulation, supposing the latter

to possess an active function by which fluids may be derived respectively, by the venous and absorbent systems. These modes respect arterial fulness; the latter may also be a cause of delay, or accumulation of blood in the veins. 4. Increased action of the capillaries of the returning system, supposing them to possess a function of this kind, by which they might suffer distention. 5. An impeded return of venous blood, by obstruction on the right side of the heart. Of these modes, it is superfluous to suggest any marks of distinction, until the postulata on which they are supposed are established on sufficient testimony. It may however be remarked, that of these modes the (1.) preternatural derivation of blood, the (2.) accelerated action of the heart, the (3.) deficient energy of the secerning or derivative function of the arterial capillaries (which as a cause of congestion is the most questionable), the (4.) increased energy of the derivative power of the returning capillaries, and the (5.) impeded transmission of blood through the heart, will all be occasionally suggested by the phenomena of disease; and the first, second, and fifth of these modes the most frequently.

It is a remark, which has been confirmed by the few cases in which I have had opportunities of examination, that serous apoplexy occurs with a quick pulse; and that sanguineous apoplexy, more especially proceeding from the rupture of a large vessel, is in connexion with a slow pulse. Many exceptions will no doubt occur to the truth of this observation.

In epilepsy I was once in the habit of bleeding, rather that patients should not die without it, than because I thought this measure of much consequence. But a discouraging circumstance once happened to me, which suggested that this motive was not strong enough to compensate for possible inconveniences. I was called to a man in the night who was in a fit of epilepsy. I proposed to bleed him, and his mother, an old woman, undertook to hold his arm: he was at this time quiet; but when he felt the puncture of the lancet, he bent his arm suddenly and violently, struck the blade half an inch into his arm, and so locked my hand in the bend of the elbow that it was several seconds before I could extricate it. Fortunately the vein I had selected was the median basilic, both at a distance from the artery and from the tendon of the biceps: this unintentional wound was followed by no ill consequences. On other occasions, both in epilepsy and in hysteria, from the struggles of the patient, the bandage has been displaced, and a considerable quantity of blood has been lost before surgical assistance could be procured, to the no small alarm of the friends of the patient, and also with some hazard to the patient, if both the vein and the orifice happen to be large.

Epilepsy is undoubtedly nearly allied to apoplexy. I never met with more than two cases in adults, in which an affection, which was decidedly epileptic in the beginning, became apoplectic in its course. This did not happen for want of bleeding, for the lancet was in these cases freely

employed, both in the commencement and as the chief means of chronic treatment.

Epilepsy, in those who are disposed to it, frequently occurs after a debauch, or unusual stimulation, whether by eating or drinking. I have reason to think that the fit might be prevented by bleeding and a purgative, if its occurrence can be anticipated. A young man, who was subject to epilepsy at distant intervals, was taken suddenly with violent pain in the head, and the usual precursory symptoms of an attack. I saw him late in the evening, immediately on this notice; and found his pulse, which was in general between seventy and eighty, a hundred and thirty in a minute. I took twenty ounces of blood from him, and gave him a dose of calomel and jalap, which operated copiously in a couple of hours. The next morning his pulse, while he was in bed, was sixty, and it was believed the fit was prevented.

Epilepsy may in general be prevented by occasional bleeding and great temperance in diet; but if the disposition to epilepsy is accompanied by chronic symptoms of affection of the head, these symptoms are not mitigated, but frequently increased by a course of depletion, which might be instituted for their cure. The truth of this remark has been confirmed by many examples. A young man of a thin habit, about eighteen years of age, had an epileptic fit, from which he recovered. A few months afterwards he was walking behind a dog: on a sudden he fancied that he saw two dogs; and this creative vision so increased, that the one dog was presently multiplied to fifteen

or twenty. This symptom became chronic: he had soon after its commencement one epileptic attack; afterwards, for about two years, all objects were multiplied; and if he looked into the air, it appeared as if he was contemplating a waterfall. He was bled repeatedly for this affection, from the arm, by leeches, by cupping, from the temporal artery, took purgatives, was blistered, had a seton in the back of the neck, &c. The more that was done to him the worse he got. He determined to take the case into his own hands, and from that time addicted himself to drinking, and rarely passed a day without taking a quart or three pints of porter. During this self-treatment the affection of the head nearly left him: his vision became almost natural, he gained flesh, and never, to my knowledge, had an attack of epilepsy afterwards. The benefits of this change of plan may perhaps be on a principle which is frequently exemplified, namely, that in our treatment of diseases we can oftentimes aspire to no more precise a design than that of changing the state of disease; and this is frequently accomplished by employing measures in succession of a diametrically opposite character. It thus frequently happens that the practitioner who is called to a chronic case, which has been before treated by depletion, gains the credit of a cure by going right about, and employing stimuli with a full diet; the success of which is probably owing to the previous depletion, which has prepared relations for recovery under an opposite plan.

A young woman of a full habit had attacks of

epilepsy, she was greatly alarmed, and a violent course of bleeding was instituted. A week rarely passed for two years, in which she did not lose blood, once, twice, or three times, and she took neither animal food nor fermented liquors. In the course of this time she suffered almost constantly from intense pain in the head; the catamenia, which, before this course of bleeding, had been regular, were suspended, every function was disordered, the blood was chiefly serum, and her feet became œdematous towards the evening: the most trifling excitement would raise the pulse from eighty to a hundred and twenty; and this state was accompanied with great heat of the scalp, pale face, and bounding of the carotids. The effect of the bleedings was to give an hour's relief from the pain in the head: and to obtain this, it was frequently resorted to, although the pain appeared afterwards to be increased. This is one among many cases in which I have observed that the frequent loss of blood increases the irritability of the heart, which is almost always accompanied by the symptoms of determination to the head. The practice is not liable to this objection, if the bleedings are moderate, and repeated only upon such exacerbations of symptoms as are distant, and may appear to demand this measure of relief. In many of these cases, it has appeared to me that the only chance of restoring a good state of health is by a gradual repletion, with attention to the bowels, and the occasional check of a bleeding. During this change of the system the blood will perhaps become buffed: the

effects of the treatment require to be carefully observed, and the treatment suspended, or its tendency counteracted if it appear to be prejudicial, or to exceed certain bounds of increased disorder. This patient got rather better under an imperfect trial of a regimen of repletion. I am uuacquainted with the event of the case, but presume it terminated in dropsy.

Chorea, which in its pathology is probably allied to epilepsy, I have no reason to think much benefited by blood-letting. In the few cases which have occurred to me of this disease, some of which were severe ones, it has yielded to purgatives in about three weeks. The purgatives were chiefly combined ones of calomel, aloes, scammony, salts, senna, jalap, and rhubarb, given so as to procure three, four, or half a dozen stools every day: the stools have commonly assumed a healthy appearance towards the end of the above period, and the spasmodic action of the muscles has quickly ceased. For the use of purgatives in this, and many other diseases, but chiefly for directing the attention of practitioners especially to the general efficacy of this treatment, the profession is greatly indebted to Dr. Hamilton of Edinburgh.

The appearance of the blood, to some extent, furnishes a rule of the propriety of blood-letting. In doubtful cases, it is a good plan to take a small quantity of blood, merely to ascertain whether symptoms that are anomalous are connected with inflammatory action. I have found the blood highly buffed where this circumstance was little

suspected; and have accordingly cured cases by depletion and low diet, which had for months resisted a practice instituted without a knowledge of this circumstance. Not that bleeding will, in chronic cases, always subvert the actions which dispose to this state of the blood; on the contrary, after a protracted course of bleeding, the blood will be as much buffed as it was in the beginning. This state will often yield to the alterative doses of calomel, with sarsaparilla, producing a slight affection of the gums; or it will often yield to a change of diet, consisting of a slow repletion, together with bark, and an occasional purgative.

The crassamentum of the blood seldom exhibits fibrine in nervous disorders of the head, even though it should be taken during an exacerbation, accompanied by a pulse of a hundred and twenty in a minute. Bleeding, in nervous disorders of the head, is sometimes indispensable as a measure of relief; but, if frequently repeated, the irritability of the heart is generally increased, as well as the local affection of the head. It is a measure of relief which may be resorted to upon a severe exacerbation of symptoms; but such patients enjoy the best health when they are brought to bear a full diet. The checks afforded by bleedings and purgatives may be resorted to, if necessary; but I have known patients, who could live comfortably with, perhaps, two bleedings in a year, under a liberal regimen, consisting both of animal food and malt liquors, who, under a reduced, and chiefly vegetable, diet, with water, could not dispense with

less than half a dozen bleedings in a year, and habitually suffered from pain in the head in a greater degree.

The most common symptoms of nervous disorder of the head are pain, throbbing, sense of tightness across the forehead, rather white, but moist tongue, and the pulse, during an exacerbation, between ninety and a hundred; at other times, perhaps, between seventy or eighty. What the precise seat of the pain might be is, in many of these cases, doubtful. I have often thought it to be in the scalp, from the great soreness felt in this part after the pain has ceased; and also from the circumstance, that a handkerchief bound for half an hour so tightly around the head, as to intercept, or diminish the flow of blood to the scalp, will often afford a temporary cure. In what proportion of cases this seat may be supposed, or by what circumstances it may be distinguished, I do not at present possess facts enough to determine. It is, however, to be remarked that, on the application of such pressure, a sense of bursting is felt in the vascular system of the head; each systole of the heart distends sensibly the vessels on the side of the pressure which is nearest the heart: this sense of distention commonly abates in five or ten minutes; and perhaps at the expiration of half an hour, on withdrawing the handkerchief, the head feels almost benumbed, and afterwards hot, but without pain. The practice of preventing the circulation in the scalp in this way may, perhaps, not always be free from hazard, if the seat of the disorder is within the cranium.

Cupping, in nervous disorders of the head, frequently gives more relief than general bleeding, and four leeches on the temples, or the loss of half a dozen drops of blood from the nose, relieving the sense of vascular distention, will be followed by a remission of the paroxysm, as complete, perhaps, as if twenty ounces of blood had been taken from the arm. Indeed, as before observed, copious bleedings, or frequent bleedings, tend generally to increase the severity of the symptoms afterwards, although they may give temporary relief.

I have seldom known symptoms of this description yield to either a course of bleeding, or to a course of purgatives; but I have thought both these courses, whether singly or mixed, as well as a limited treatment by blue pill, although at the time, perhaps, productive of an increase of the symptoms, beneficial, by disposing to a recovery of health under an alteration of plan, which by degrees may consist of a regimen of full repletion. This change of plan I have known followed by boils; and the bleeding itself, in nervous cases, has sometimes produced an excitation, or inflammatory diathesis, under which boils have occurred. A tendency to cutaneous disease is not unfrequently in connexion with what is termed nervous disorder; and the form of cutaneous disease is, I think, most frequently that of erysipelas. The boils which occur in this connexion of disease, I have thought best treated by a full diet, with, perhaps, bark, and occasional purgatives; or by six grains of the extract of colocynth, and one or

two of blue pill every night; and if they get well under this plan, the improved state of health which succeeds is often permanent.

Chronic disorder of the head is most frequently accompanied by dyspepsia. It is supposed to be dependent upon dyspepsia. I cannot myself agree, without reserve, in this opinion. The reason why pain, throbbing, &c., in the head succeed to a meal, is, that the brain is disordered previously to the meal: it is a state of predisposition, which the stimulus of a meal serves to excite; and hence a preternatural derivation of blood to the head. The heart also may be concerned in producing this effect; the pulse is generally quickened by the stimulus of the ingesta. Thus, the state of the stomach, of the heart, and of the brain, form a circle of relations; and it is difficult to decide on which the phenomena displayed in these organs primarily depend, or the share in these phenomena which might be imputed to each. There is scarcely in the symptoms of the disorder of these organs, in some instances, a definable order of succession; or, the quickened pulse, the pain in the head, &c., are synchronous: in other cases the action of the heart is first quickened, and throbbing, then pain in the head, ensue. It suggests itself in some cases, that a full stomach may, by impeding the descent of the diaphragm, limit the expansion of the lungs during inspiration: if this effect were produced, the blood on the right side of the heart would suffer a remora, from diminished pulmonary circulation, which would be extended to the blood in the returning systems of

the venæ cavæ. It is probable that the effect of such an impediment would be but trifling in ordinary cases; this, however, may be adverted to among possible concurring causes. Frequently the affections of the head which are in connexion with disorder of the stomach, do not succeed immediately to a meal; but suffer at this time a perfect suspension, and come on when the business of digestion may be supposed to be over, as on the following morning. The relation of disorder between the head and the digestive organs will be more fully considered under the latter title.

The nervous affections of the head, just alluded to, require rather a preventive management than a curative treatment. We may succeed with the former design, but shall certainly fail in the latter. Bleeding is no curative remedy; the same may be said of emetics, purgatives, nauseating medicines, blue pill, lotions to the head, setons, blisters, temporary or perpetual; even a typhous fever, or comparative obesity, are not always adequate to the cure of this form of disorder. Yet these remedies, although not curative, may be resorted to in the preventive or mitigating treatment of the case. They will most of them give relief, if employed during an exacerbation of the symptoms: if pushed further afterwards, with a view to a cure, they will generally increase the symptoms; but they will also dispose to a better state of health, when this plan has been carried to a certain extent with a design of curing, is found to fail, and a guarded treatment by repletion is substituted for it. The results of the treatment are, of course, irregular in

different examples: these means will sometimes regularly improve the state of disease to a final recovery, without either increased or additional symptoms; but this is not their common effect, so far as my experience has gone in this form of disorder, and this experience has been pretty considerable.

Among the most obstinate and severe affections of the head which I have met with, have been a few cases, in which disorder of the head appeared to substitute a disease of the skin, or of the scalp. In two cases of this kind, in which painful disorder of the head succeeded to the cessation of an eruptive disease of the scalp, every sort of treatment has been carried to its greatest possible extent, and tried in vain. Bleedings from the arm, from the temporal artery, by cupping, by leeches, blisters on the back of the neck, between the shoulders, on the scalp, behind the ears, cold lotions, hot fomentations, setons, pustular eruptions produced on the scalp by emetic tartar ointment, the extraction of suspected teeth, nauseating and emetic medicines, a long course of purgatives, tonic remedies, as steel, bark, arsenic, ammonia, assafœtida, opium, &c.; vegetable diet, and starvation; animal diet, with ale and porter—The affection of the head has continued uninfluenced by any of these means—in one of the cases alluded to, it at length terminated completely by metastasis to the liver, which viscus became considerably diseased, and nothing was gained by the change of seat: in the other, the same form of disorder of the head

continues; and has continued, with but trifling and occasional mitigation, more than ten years.

Determinations of blood to the head in women, which are vicarious to menstruation, are not uncommon. I have never known apoplexy occur from a determination of this sort; hysteria is a frequent effect of it: and if the suspended menstruation is chronic, the symptoms are frequently pain in the head, with occasional hysteria, and spitting of blood. In one instance I have known repeated attacks of epilepsy occur in connexion with a chronic suppression of the catamenia, and this patient never had hæmoptysis.

A striking relation of disease, accompanied by a disposition to the derivation of blood in the head and chest, is exhibited in the following case: A man of rather a full habit, and pale complexion, about forty years of age, by trade a baker, was attacked by violent pain in the head. He was bled repeatedly from the arm, from the temporal artery, by cupping, by leeches, took purgatives, was blistered, &c. This treatment, which was continued five or six weeks, scarcely produced any mitigation of the symptoms: in the course of it, and towards the end of the above period, he had a copious spitting of blood, which was sometimes frothy, was coughed up, and evidently proceeded from the lungs. As soon as this hemorrhage occurred, the head, which had not been relieved in any degree by previous blood-letting, continued up to this period, became perfectly free from pain. The expectoration of blood continued a few days; it

ceased; and the pain returned in the head with unabated violence. For some weeks afterwards, the seats of affection alternated with almost perfect regularity; one day, pain in the head; the next, hæmoptysis. The disorder of the head eventually prevailed; the hemorrhage from the lungs ceased; the man became mad; he was sent to a lunatic asylum, and died of apoplexy. This case, in connexion with some preceding ones, shows forcibly how inadequate blood-letting is for the prevention of disease, of which determination of blood is even a conspicuous sign, unless it has a curative relation with the state of disease, upon which the symptom of preternatural determination depends.

There is rarely, in hæmoptysis, any one symptom by which, in the commencement, the seat of the hemorrhage, if copious, can be discriminated. The act of vomiting is common in hemorrhages which proceed from the lungs, as well as at the time of copious expectoration of mucus, in catarrhal affection of the bronchia, and is occasioned by the irritation of the larynx, fauces, &c., incident to such expectoration, and a sympathetic action of the diaphragm in consequence. Hence, the inquiry in cases of spitting of blood, whether there was any vomiting, is generally answered in the affirmative. The lungs will, however, be indicated as the seat of hemorrhage, in some cases, by the frothy appearance of the blood; in others, by its being expectorated with mucus; and almost in all by its being unmixed with the usual contents of the stomach. At the same time, the mixture

of the contents of the stomach with the blood, or even the succession of dark-coloured stools, is not a proof that the hemorrhage is from the stomach; for hemorrhage from the lungs may be accompanied by actual vomiting, and a considerable quantity of blood may also be swallowed, giving its colour to the stools. This, however, is rarely a point of diagnosis of any great difficulty: if the seat of hemorrhage is not proved by any one sign, it is by a history of the case, by a combination of symptoms, or by some collateral indication.

I have had reason to think saline purgatives, with sometimes calomel, nitre, ipecacuanha, &c. more effectual in hæmoptysis, than repeated blood-letting. Spitting of blood frequently ceases as soon as nauseating doses of emetic tartar produce full vomiting; but I have known an emetic bring on a copious spitting of blood, which continued several days in a person with whom this symptom had for some time been suspended. It may be remarked of emetics, in affections, whether of the head or chest, that they are powerful, and very useful remedies, if they do no more than the practitioner intends: but the act of vomiting, impelling blood with additional force, perhaps upon a seat of preternatural determination, is hazardous; and I have often felt relieved from apprehension, when it might be calculated that the action of an emetic was over.

Blood-letting in hemorrhage from the lungs may, if employed with discretion, be highly beneficial; but, as in affections of the head, if carried to a great extent, it is likely to increase, rather than

diminish, the unequal distribution of blood, and the seat of preternatural determination in this, as in other instances, suffers additionally from this effect.

In cases of hæmoptysis in females, connected with suspended menstruation, it is generally, and with propriety, an object of treatment to restore the catamenia. The most effectual mode of doing this is, I believe, by adequate purging, with aloes and camboge; the uterus sympathises with the violent and constant irritation of the rectum, which may be produced by these means; and by participating in this irritation, a derivation of blood is produced, which commonly terminates in menstrual secretion. These means would be properly instituted with most vigour between the periods of hæmoptysis. The warm bath, or hip bath, is a doubtful remedy; it may determine to the lungs, or to the uterus; and, accordingly, I have known it sometimes followed, in conjunction with other measures, by menstruation, or by a return of hæmoptysis, in an increased degree. The same may be said of steel, and other medicines which quicken the circulation. Leeches, within the labia, in a limited employment of them, in this way, have not, in my experience, produced any benefit. I have reason to think the occasional distention of the rectum, by hot water, an useful measure, as auxiliary to the action of purgatives. Whether the irritation of the rectum by the employment of the bougie may be advantageous, I have no experience to decide. It was a practice of remote date to attempt the restoration of the catamenia

by means of pessaries*. Possibly, medicated applications, in some such form, of a highly stimulating quality, to the os uteri and vagina, may assist in deriving blood to this system. It is probable, however, that the pessary eulogized by Dioscorides, viz. one made with the juice of mandrakes, would not at this time be selected.

The pathology of hæmoptysis furnishes two important practical distinctions, namely, hæmoptysis with, and without buffed blood. If blood, taken from the arm, in cases of hæmoptysis, is buffed, the local disease tends to rapid disorganization; and unless arrested by treatment, terminates speedily in consumption. If, in such cases, the blood is not buffed, there is no immediate danger of consumption; the only danger is from hemorrhage: and this danger is not trifling. The most inconsiderable spitting of blood is always a symptom which requires great attention. If a small vessel of the lungs is ruptured, it proves a disposition in the vessels to this effect; and there is no security, but that a large one may next give way. A young man, about twenty years of age, had a pain in his side, and sometimes expectorated a very little blood. He took no notice of these symptoms, and continued his work as usual. One morning he returned from his accustomed labour, and whilst at breakfast a copious hemorrhage from the lungs took place: I was immediately sent for, and arrived at the house in a few minutes; but the young man was dead before I got there; the

* Sir Thomas Brown's Vulgar Errors, p. 289.

floor was covered with blood, and it appeared that the rapidity with which the effusion of this fluid had taken place had produced suffocation.

In hæmoptysis with buffed blood the symptoms cannot be too rapidly subdued by antiphlogistic means. Bleedings of sixteen or twenty ounces, calomel, active saline purgatives, full doses of nitre, squill, ipecacuanha, emetic tartar, &c. and a diet of cold gruel, tea, barley water, &c. should be employed in the beginning, together with perfect repose. If the case becomes chronic, small bleedings, as a collateral treatment, are advantageous, together with nitre, and constant purging by Epsom salts: I have ample reason for thinking the repetition of large bleedings prejudicial; and a treatment, in which blood-letting is made the only remedy, altogether inadequate. I have known all the symptoms cease, in these cases, and the blood has no longer exhibited fibrine, when the calomel, which was given in doses of three grains every night, followed by salts and senna the next morning, has affected the gums. I have known also the coagulation of the crassamentum gradually weakened, and the exhibition of fibrine cease, under a treatment by a grain of acetate of lead three times a day, in combination with a full dose of squill, and a small one of digitalis; the saline purgatives, and occasionally calomel, have also been employed at the same time.

If the blood is not buffed in hæmoptysis, the immediate danger arises from hemorrhage; but a portion of unabsorbed coagulum will, it is currently believed, at a remote period, form the nu-

cleus of a tubercle: this will be followed by fever, local disorganization, and in this state, as I believe in every other case of consumption, the blood will be found buffed.

The best treatment of this form of hæmoptysis is also, so far as my experience has gone, to subdue the symptoms by smaller bleedings than in the former cases, by saline purgatives, nitre, squill, ipecacuanha, emetic tartar, &c. and occasionally calomel as a purgative, not frequently repeated. In this case, in which there is no inflammatory action to be subdued, I have no reason to think that any thing would be gained by mercurial affection: on the contrary, it may convert hæmoptysis without fever into that febrile form in which buffed blood would most probably be exhibited. Repose, with vegetable diet, and that nearly cold, with a temperate atmosphere, are also necessary in this simple form of hæmoptysis. I have known the chest covered constantly with cloths dipped in cold vinegar and water: but from experience, I have no reason to think this practice beneficial: and from principle, I should not be inclined to anticipate from it any good results. I once knew a case of pneumonia treated by cold applications to the chest: the boy, who was about fourteen years of age, died; and certainly with a moderation of symptoms, from which I should not apprehend much danger under the ordinary treatment.

The chronic treatment of hæmoptysis without buffed blood is sometimes by moderate repletion; and even wine, to the amount of two or three glasses every day, has made part of a medical

direction. I am not acquainted with the events of the cases alluded to, which were treated in this way; but during a twelvemonth or more, in which this plan was pursued, the patients had frequent spittings of blood, lost flesh slowly, and suffered more or less every day from fever. Upon the whole, the first object is to subdue the symptoms, and this, I believe, is best done by the depleting and antiphlogistic plan above mentioned. The next object is to prevent their recurrence; with which design this treatment should be continued some time after the symptoms have ceased, and slowly moderated. After a long interval of freedom from the symptoms, if the former diet is resumed, it should be by very slow degrees; its effects should be watched for a very long time; and repletion, and the tendency to pulmonary determination, kept in check by small doses of Epsom salts repeated at least twice or three times a week for some months after the cessation of the symptoms, together with abstinence from violent exertion, stimulating food, hot fluids, or, in short, any thing which tends to quicken the pulse.

In determination to the lungs, without organic disease of the heart, producing continued, or temporary asthma, I have found a combination of nauseating, emetic, and purgative treatment, together with the application of blisters, or what is more effectual, though less speedy in their action, of plasters to the chest, containing emetic tartar, more beneficial than bleeding. Moderate bleeding in the beginning may be useful, and often is so, in a striking degree; but I have known an

attack treated by bleeding last many weeks longer, although of the same kind, and in the same individual, than one treated by purgative and emetic medicines. The form of medicine from which I have seen the most palpable benefit, in a person of a full habit, about forty years of age, who had once a year an attack of asthma, which sometimes lasted three months, and ended in a catarrhal defluxion from the bronchia, was a combination of two grains of calomel, half a grain of elaterium, three grains of squill, half a grain of emetic tartar, and three grains of aloes, in two pills, taken every eight hours, with perhaps one draught of salts, senna, and jalap, if the bowels were not speedily affected by the pills. These doses could not be continued more than two days: they were then reduced, but still their first influence of nauseating, vomiting, and purging, was afterwards kept up; and a form of disease which used to continue three months, ceased in less than three weeks.

I have reason to think issues beneficial in habitual asthma, which is not connected with organic disease of the heart: the best situation for these seats of suppuration is on the inside of the thigh, a little above the knee, in each leg; or an issue in each arm. One issue, containing one pea, may be beneficial so far as its influence extends; but it is generally inadequate to produce any decided effect upon a disease. I have never employed setons on the side or chest in asthma; but the report is unfavourable from some who have tried them: they are sources of too much irritation,

where the object is to subvert disease by a constant and unirritating suppuration.

In speaking of one form of congestion affecting the head, it was observed that this occurred in some persons only after a meal, proving its connexion with the state of the stomach: and by the facts quoted in support of this remark, it was suggested that the cause of this congestion of the head was a spasmodic affection of the heart from sympathy with the stomach, impeding the passage of blood through its right side. It is probable that many, we can perhaps scarcely define accurately the proportionate number, of the cases of spasmodic asthma, proceed from a sympathy with the digestive organs, similar in character, but affecting the left side of the heart, by which the return of blood from the lungs is impeded. The sympathy between the heart and the stomach has been insisted upon with much justness by Mr. Abernethy; and it goes probably some way in explaining the benefits of purgatives and of nauseating medicines in severe cases of this form of asthma. I knew a lady who had occasional attacks of asthma, which did not continue many hours, but they were so severe as to threaten suffocation. The attack once or twice ceased immediately upon opening a vein in the arm; but it recurred very soon afterwards. This affection of the chest was accompanied with black stools: the disease was cured by a few purgatives with calomel, jalap, salts, senna, &c.; and by taking afterwards, as a preventive treatment, three grains of aloes, and three of squill, every night for about two months.

A limited diagnosis with respect to the diseases of the heart, which concern the pulmonary or general circulation, is not difficult. The diseases of this organ may be said to be organic, and those only of action. The latter are specific, or sympathetic: specific disease of the heart, consisting of disordered action, is exemplified in inflammation, or rheumatic affection, of this organ, and sometimes, probably, also in some irregular actions, without accompanying disease: but the most frequent affections of the heart are the sympathetic ones; and in these cases the primary seat of disorder may be any part of the system, but is most frequently either in the stomach, or brain, or both. These affections are commonly exemplified in the form of asthma, and in other congestions, before imputed to an impeded transmission of blood through either side of the heart, from this sympathy: and in a few instances of the palpitations which are called nervous, and are unaccompanied by dyspepsia. The pathology of these latter instances belongs to the subject of related disease.

The same phenomena as those which characterize the organic diseases of the heart sometimes occur, and continue only for a short period. If such disturbance of the circulation is only temporary, and followed by a perfect interval before its recurrence, of months, or weeks, or even perhaps of days, it is fair to presume that the affection of the heart is only one of action, since a change of structure is a mechanical cause, and would produce permanently the same phenomena in a degree, which might be increased or diminished

by the presence or absence of exciting causes, producing disorder of action, which concurs with the mechanical change.

Nearly all, certainly the principal diseases of the heart, and vessels proceeding immediately from it, tend to impede the passage of blood through this organ. The order of symptoms is, I believe, generally, and justly, considered a sufficient proof of the side of the heart which is the seat of organic impediment. If the passage of blood through the right side of the heart is obstructed, the symptoms will be congestion of the head, perhaps of the liver, ascites, and anasarca of the extremities; the impediment on this side of the heart being to the return of blood from the venous systems terminating in the cavæ. If the lungs first suffer congestion from disease of the heart, there is little doubt but the obstruction is on the left side of this organ, by which the passage of blood through it from the pulmonary veins is impeded. It matters very little, so far as diagnosis is attempted to this extent, which is sufficient for many practical purposes, what the precise seat or nature of the obstruction might be; whether a contraction of the auriculo-ventricular opening, or an ossification of the valves of the aorta, the return of blood from the lungs would be alike impeded, and these organs would in either case be the first to suffer from congestion.

But a disease commencing on the left side of the heart, and occupying this seat for a considerable time, may be fatal eventually, by producing congestion of the brain and apoplexy; since the

congestion of the lungs might extend even to the heart itself; and if the return of blood is impeded by obstruction on the left side of the heart, the escape of blood from the right side of this organ through the pulmonary artery would also be impeded; hence the passage of blood from the venæ cavæ, through the heart, would be obstructed, as if the disease were on the right side. This impediment is, however, of an indirect kind: and a security against this secondary effect of disease on the left side of the heart, is afforded to a considerable extent by the dilatability of the pulmonary vessels. Of the phenomena dependent upon states of disease of the coronary arteries, I have no experience; and can therefore have nothing to suggest from my own observation.

Dr. Parry has connected a double stroke of the heart, which occurs in some diseases, with a state of this organ similar in effect, to that of an organic contraction of the auriculo-ventricular opening. This double stroke of the heart Dr. Parry imputes either to an impeded entrance of blood from the auricle into the ventricle, or to a regurgitation of blood from the ventricle into the auricle, at each systole of the ventricle; giving a pulse in the auricle, which alternates with that of the ventricle*. The double stroke of the heart had also previously been imputed by Mr. Hodgson to contraction of the left auriculo-ventricular opening†. On this explanation it might be observed, that, if such a state of the

* Inquiry on the Arterial Pulse, p. 143.

† Treatise on Diseases of the Arteries, p. 33.

auriculo-ventricular opening occasioned this precise alternation of stroke, we might expect it to occur in those marked cases of organic disease of this kind, in which the opening from one into the other of these cavities is reduced to a small fissure. Yet in none of the histories of the cases of organic disease of the heart of this kind, and in this degree, with which I am acquainted, has this precise alternate stroke been remarked. In an extreme case of this kind, the motions of the heart, and of the pulse, are described by Corvisart in nearly the same terms*. As examples of this affection are not common, the following, which occurred to me, and first induced me to speculate on the subject, may be briefly mentioned. The patient was about forty years of age; he had suffered in the course of his life two or three attacks of acute rheumatism, which showed always a great disposition to metastasis, and his heart was more or less affected in each attack. About two years since he had acute rheumatism, affecting chiefly the arms and legs; during this attack he was under my care. In the course of the disease the swelling of the hands sometimes ceased suddenly, and was succeeded by violent pain in the heart, sense of weight on the chest, and of suffocation, with a quick and intermittent pulse. Towards the termination of the case, the stroke of the heart was precisely double that of the pulse at the wrist. When the pulse beat fifty, the heart beat exactly a hundred strokes in a minute; when the heart

* On Diseases of the Heart, Hebb's translation, p. 178, &c.

beat eighty, the pulse was forty. The case was chronic, and my examinations frequent, and the result was always the same: the pulse of the heart was invariably, to a single stroke, double that at the wrist; and there was in general no very perceptible difference in point of strength between the beats of the heart; or if at any time a difference could be felt, it appeared as if the second stroke was rather the weaker of the two.

The most simple explanation of this phenomenon which occurred to me was, that the sympathy between the sides of the heart, which occasioned them in general to act together, was interrupted by rheumatic affection of this organ; and that the systole of each side, instead of being synchronous, was successive. A contracted state of the opening between the auricle and the ventricle was certainly in this stage of the case not suggested by any consequent congestion either in the lungs or elsewhere; and in an earlier period of the case, when disordered action of the heart was connected with great difficulty of respiration, the pulse was extremely irregular, both of the heart and at the wrist; but there was nothing like a precise alternation of stroke. The subject of the above history recovered, and has since enjoyed good health; but his pulse always intermits after exercise, without any accompanying symptoms of disease.

In addition to the diagnosis of the seat of impeded circulation, afforded by congestion of the returning system of blood vessels, other signs are also furnished by the deprivation of blood in the system of vessels which convey blood to parts.

Thus the organs of respiration may suffer a distress from the deprivation of blood, by aneurism on the right side of the heart, equal to that which is produced by a congestion, in consequence of an impeded return of pulmonary blood, by obstruction on the left side of the heart. This circumstance, although affording an additional sign, gives rather a complexity to the diagnosis: to obviate the confusion which might arise from it will, however, require only a more strict scrutiny of the order of symptoms.

To what extent, or on what occasions, congestion may be imputed to deficient energy in the secerning system, it is difficult to pronounce. The heart and the capillaries concur in the circulation: if the action of the heart is preternaturally vigorous, it would appear, at first sight, probable, that blood, being sent to the secerning system in an increased ratio to the separations from it, the vessels which precede this system in the order of the circulation, may suffer a preternatural fulness. But it is doubtful whether this fulness would take place to any extent; since, in a paralytic limb, or in one the death of which gradually precedes that of the principal organs, the derivative power of the capillaries having nearly ceased, congestion does not take place from the continued action of the heart: on the contrary, all the blood impelled into the vessels of such a part is readily disposed of by the veins, and the vessels are preternaturally empty. If, also, there were a local defect of this derivative power, the blood would want an auxiliary force to the circulation in such part, which, prevailing else-

where in its natural degree, such part would not be likely to obtain more blood than would be disposed of, even by the mechanical arrangement of vascular continuity.

Vascular fulness is more likely to occur in the lungs from impeded circulation in the capillary system than in any other seat, both on account of the contiguity of this structure to the central organ of the circulation, and because the system of vessels in these organs being derived from one trunk, the resources of collateral circulation are fewer than elsewhere. As the office of the lungs exposes them to the vicissitudes of the atmosphere, almost in as great a degree as the skin is exposed, it seems not improbable, if the doctrine of capillary spasm may in any case be admitted, that it is more likely to occur in the lungs than in any other structure. It is unnecessary to resume this question at any length, which, even in this instance, would admit considerable theoretical discussion: and this may be the more readily dispensed with, as the anatomical arrangements, and still less the precise relations of the different orders of capillary vessels, are not understood: they are, however, of importance sufficient to demand an exclusive investigation, by which a satisfactory developement of their physiology appears by no means hopeless.

Hydrothorax, and effusion into the pericardium, are less successfully treated by bleeding than by purgatives; of course, I speak only of what has happened in my own experience. The effects of purgatives in relieving patients from the pressure

of fluid on the heart, lungs, and diaphragm, are almost magical. A woman, about sixty years of age, had hydrothorax, connected with disease of the heart, for many months. When I first saw her, she was sitting upright in her bed; her breathing was exceedingly laboured, her pulse intermittent, and only thirty-six in a minute. Ascites had of late been added to hydrothorax, and I found her legs tightly bandaged to prevent their swelling. The woman appeared almost in articulo mortis: the bandages were of course removed. I covered her chest with a blister, and gave her a full dose of calomel, squill, aloes, and elaterium, followed by a black draught with jalap. The alvine evacuations were, in consequence, very copious; the kidnies afterwards acted inordinately, and she passed many pints of almost limpid urine every day. In less than a week, under repetitions of the same medicines, she was totally free from all her symptoms. The plan was continued in less degree, by way of prevention; but she was well enough to walk out, and became tired of discipline. Her pulse, after the first three days of this treatment, averaged seventy: it still intermitted slightly, and the perceptible action of the heart itself indicated that this organ was not free from disease. She remained well, that is, free from effusion; in a few weeks had a relapse, was treated in the same way, with the same immediate relief; she afterwards had one or two other attacks, with the same results of treatment; and eventually, refusing any longer to prolong her life by medicines, she became comatose, and died of apoplexy. It is worthy

of remark, that in connexion with this disease of the heart, the cornea of one eye became opaque; it was gradually projected considerably beyond the eyelids; it appeared black, like a semitransparent tumour, containing venous blood; and if she had lived longer, it was thought probable that the eye would have assumed the state of a bleeding fungus.

Idiopathic dropsy, or that supposed to arise merely from preternatural fulness of the vessels of the peritoneum, commonly yields to purgatives, as of calomel, elaterium, camboge, with perhaps large doses of squill, and small ones of digitalis. I have known bleeding employed without any obvious effect, either good or bad: the cure may generally be rested on purgatives, of which calomel makes a part; and it most commonly happens that some degree of ptyalism is produced by the calomel, before the disease is completely removed. I believe that this form of dropsy may almost invariably be cured by these means; and I should never think of tapping, if it could be avoided; it is at best but palliative; and medicines, if properly directed, may almost with certainty do as much, and have, at the same time, a curative tendency with respect to the disease.

The employment of mercury in dropsy has been an empirical practice; but its powers deserve all the attention which observation and science can bestow. I believe mercury will very frequently cure the disease, if this remedy is carried to a great extent. But it is often difficult in this, and other local diseases of a fixed character, to make mercury produce its usual effects; and upon its pro-

ducing salivation, I have reason to think its curative operation depends. Not that the mere salivation is of any use; but it is an index of a more general affection; and I have never met with a case of dropsy, unaccompanied by disease of the heart, which did not cease under a salivation, continued for a sufficient length of time. It has been proposed to limit the use of mercury to those cases of dropsy only which are not inflammatory. I have used it with the most perfect success, where the blood has been buffed; and I have certainly found a treatment by bleeding fail, although the blood always exhibited a surface of fibrine. But my experience on this point has not been sufficiently extensive to decide so important a practical question.

In symptomatic dropsy, the danger does not arise from the mere effusion, but chiefly from the visceral disease which produces or accompanies it. The visceral diseases which produce dropsy are commonly those of the heart and of the liver: the treatment of the former can only be palliative; that of the latter may be curative. Dropsy is here imputed to disease of the liver, conformably with the prevalent theory of a mere hydraulic relation, by its occasioning mechanical obstruction to the passage, through the liver, of the venous blood returned from the other abdominal viscera. But when it is considered that ascites is sometimes an idiopathic disease, and that at other times every grade of diseased liver, from induration and enlargement, even to an extensive destruction of this viscus by abscess, occurs without producing dropsy, it may

be suspected that the effusion connected with organic disease may be dependent upon an associated disposition to this effect, which prevails in the secerning system, or else upon a derangement of the antagonist function of the absorbents. At the same time, the frequency of the connexion suggests that the organic disease must have at least the force of a concurring cause, or one which excites a predisposition.

The cure of symptomatic dropsy, in connexion with disease of the liver, is no trifling illustration of the powers of medicine; and the medicines which will accomplish this end are of no very gentle kind. An example of the direction of these powers is furnished in the following case:

A woman, in other respects of a good constitution, between forty and fifty years of age, suffered occasionally from chronic inflammation of the liver. This viscus was, at these times, enlarged, indurated, and painful, on pressure: she had three or four of these attacks in the course of two years, and they yielded to the ordinary treatment in a fortnight, three, or six weeks; and towards their termination, the stools were sometimes copious, and almost black. She was subject to more frequent attacks of bilious vomiting and purging, which commonly ceased after one or two doses of calomel, followed by a cathartic draught. During the above period she suffered only occasional inconvenience, and very little confinement from her complaint. The liver again became enlarged and indurated; the disease did not, at this time, yield to ordinary remedies: she was purged, bled, leeched,

cupped, and blistered, repeatedly; the tumor in the side continued to increase, her legs became anasarcous, and fluctuation was perceptible in the abdomen. She took principally purgatives, and the topical treatment of the side consisted of local bleedings and blistering: she took small doses of calomel, as a grain, three times a day, and rubbed in mercurial ointment upon the side. This plan was continued nearly three weeks, and the quantity of calomel, during this time, was increased to six or eight grains a day. The gums were not, however, in any degree affected; the enlargement of the liver continued to increase; the anasarca of the legs extended from the feet to the hips, or to the sides of the abdomen: her hands and arms were also anasarcous in a slighter degree; and her respiration was difficult, owing in part to the tumor of the liver, and in part to fluid in the abdomen. Her case was considered a hopeless one; and from experience of the inefficacy of the remedies hitherto employed, it was abandoned, except by myself. I told her there was still a chance left for her; that I was not very sanguine of the result; but that I would give her this chance if she chose to submit to the discipline. Her death seemed otherwise inevitable, and she therefore consented.

As the former doses of mercury had not produced the effect upon which I believed the curative action of this remedy to depend, I determined to produce this effect at all events; and as there appeared no time to be lost, I gave her ten grains of calomel every night and morning, and directed her to proceed with the mercurial frictions. She

continued these doses about a week: the stools, consisting of very depraved secretions, became copious without any other purgative, and her gums were swollen and tender. I then gave her eight grains of calomel, with squill, every night and morning, and she continued these doses about a fortnight. She occasionally took also a black draught, with half a dram of jalap in it. Her gums, during this fortnight, continued much swollen; the palate also was thickened; there was a little increase of salivary secretion, but no ulceration of the gums or tongue. She had every day copious stools, consisting of vitiated secretions of apparently every variety, black, green, &c., with a considerable proportion of mucus, and sometimes a very little blood. The tumor in the side had nearly disappeared; she could lie flat on her bed; there was very little enlargement of the abdomen, but the legs continued anasarcous. She afterwards took every night pills, with five grains of calomel, three or four of camboge, half a grain of elaterium, and three grains of aloes, followed every morning by a black draught, with half a dram of jalap in it. This plan was scarcely interrupted for three weeks; and when it was, it was only to omit the purgative, and resume, for perhaps a couple of days, the ten grain doses of calomel, when the symptoms under the purgative showed a disposition to increase. Her pulse throughout this course was, upon an average, between 90 and 100. She also occasionally took an emetic; and sometimes, by way of varying the purgative, a dose of spirit of turpentine, with castor

oil. The evacuations from the bowels were still exceedingly copious, and consisted chiefly of the secretions before described. Her diet was of the most nutritious quality. As a compensation for this severe discipline, I allowed her to gratify her palate by eating and drinking as her fancy dictated, without any restraint. My real motive for this indulgence was one founded in an experience of the advantage, in many chronic cases, of sustaining with one hand, while we are pulling down with the other. The curative design is to subvert a state of disease: and if the disease is a formidable one, and this design is really to be accomplished by medicines, without being indebted to nature, (who is in such instances driven towards our purposes very much against her will) the means by which this can be accomplished must be of no trifling powers. This discipline was continued about two months, with but little interruption; at the end of which time, as no vestige remained of her complaints, it was presumed that the remedies had subverted the diseased, and were followed by the resumption of a healthy state of the functions. I did not, however, choose to trust over much to this presumption, knowing that it is one thing to cure symptoms, and another to subvert the tendency by which they were produced, and may be produced again, when the influence of the remedy has passed. She therefore continued to take her pills, with calomel, camboge, elaterium, &c. together with the draught, containing half a dram of jalap, almost every night and morning, for two or three weeks longer: these doses were then given

every second day, every third day, and irregularly three times a week, and twice and once a week for some time afterwards. These purgative doses, it must be remarked, had, in this woman, no more effect on the bowels than weaker doses would have had on other subjects; and consequently, in many other persons, the same results may probably be produced by less violent means.

The gums, from an early period, almost to the end of the above treatment, continued very much swollen and painful, and the saliva occasionally flowed freely for two or three days. The calomel appeared to act powerfully both on the kidnies and skin. She passed daily large quantities of almost limpid urine, which coagulated abundantly on exposure to heat; and during the night she had frequently colliquative sweats. A swing was put up in her bed-room, and I thought the exercise it afforded beneficial. The case terminated in perfect recovery, and the patient has now been well more than twelve months, without having had any relapse; she has resumed her usual habits, has exposed herself freely without caution, or rather imprudently; she takes occasionally one of her accustomed purgatives when she thinks she is bilious, and has not suffered from the treatment any form of substituted disease. I could add to this, other cases of the same kind, in which a treatment has been successful which was conducted with the same views; but this addition seems superfluous, as the cases alluded to were less severe than the one just related, and the objects of the treatment more easily attained.

I do not remember to have ever seen the principle defined on which mercury acts so beneficially in diseases of the liver. If absorbent glands are enlarged, the mercurial diathesis frequently makes them suppurate; but if this does not happen before salivation takes place, or if salivation succeeds to the suppuration of such glands, as those of the axilla, for instance; in the former case, the revulsion made under ptyalism to the secerning glandular system will be followed by a reduction, or perhaps disappearance of those glands which were enlarged; and in the latter, the same mode of operation will frequently produce the absorption of matter which was formed under mercurial influence, but previously to the ptyalism. The mercurial diathesis would also, in many cases, excite suppuration of the *liver*, when this was diseased, as of other structures; but the determination to this gland, produced by mercury, instead of exciting suppuration, forces it upon an active secretion, by which its state of congestion is relieved, as congestion or determination is, in other instances, by a spontaneous, or other ending, in secretion. A quantity of mercury, therefore, which, not producing salivation, would in other structures produce or hasten disorganization, in the liver produces only a less degree of the salutary influence which this medicine exerts upon this viscus when ptyalism, or a general increase of glandular secretion, is excited by it; and if the fixed nature of the disease is a cause why mercury does not produce ptyalism, (as it is in some instances, for the reason that the particular determination of mercury is rather to a previous

seat of disease, than to the glandular system) the local disease is not, in the case of the liver, as in other structures, increased by this particular determination, but still finds, at once a security against the prejudicial operation of mercury, and a vis medicatrix, under its influence, in the secerning function of the gland. Thus, though in diseases of other structures mercury may be prejudicial if it does not produce salivation, although it may cure if it does, every grade of mercurial influence *on the liver* is likely to be advantageous in its diseases; and mercury does not in this, as in other seats, produce opposite effects in different stages of its employment. The results here attributed to mercury, and this view of its operation is not, I presume, true without limitation; variety in the disposition and states of disease will, doubtless, change ordinary relations, with the same agents: but the above description of the general tendency and dependencies of mercury has been confirmed by my own observation, so far as opportunities have been permitted me.

It is by no means a new observation on the employment of mercury, that friction alone is not to be confided in for producing a constitutional affection. Mercurial frictions may produce salivation without a corresponding effect upon the other glands. It has been known that mercurial ointment rubbed on one leg has affected only the corresponding side of the mouth; and I have seen severe salivation produced by mercurial ointment, while no proof has been afforded that the liver was influenced by mercury, either by the reduction of the swelling

of this viscus, or by a change in the appearance of its secretions. The exhibition of mercury by the mouth, with which mercurial frictions may be conjoined if necessary, comprise an employment of this remedy which appears best calculated to produce a constitutional influence.

Returning to the pathology of determination, we have seen that as a doctrine it is defective, or explanatory only of a few facts; that determination of blood, or rather derivation, when not produced by mechanical obstruction, is at most but a consequence, and a concurring cause of disease. In quality of a re-agent, it may increase disease in the department of life, or it may produce direct changes of the mechanical kind in the structures, and through these, influence indirectly the state of disease in the department of life. We have seen that the therapeutics which the doctrine suggests (consisting chiefly of blood-letting) are defective in principle, as the pathology upon which they are founded is deficient; that blood-letting, by diminishing a supporter of disease, may diminish the energy of symptoms; by its relation with the mechanical state of the vascular system it may diminish an effect of re-agency, either in the mechanical department or in that of life, or it may obviate a concurring cause of disease.

We have allowed blood-letting, chiefly on physiological grounds, to be a means of diminishing the energy of disease, by diminishing the quantity of a fluid which is a supporter of disease. This effect from blood-letting is, however, by no means universal; or, the natural tendency of the measure

in this respect may be superseded by its relation with the state of disease. Although by blood-letting we diminish the general fulness of the vascular system, yet we oftentimes do not perceive a corresponding local diminution of vascular fulness in the seat of disease. On the contrary, in some nervous affections of the head, bleeding, which diminishes the quantity of blood in the general system, seems to increase the evidences of determination to the head; and we find apoplexy occurring after perhaps the system has been deprived of more than a hundred ounces of blood, while pain or vertigo only were the symptoms before a single ounce of this fluid had been abstracted.

The cessation of disease under blood-letting depends upon *a curative relation* of the remedy with the state of disease; and as this relation can be known only by experience of the effects of blood-letting in connexion with certain states of disease, so theoretically, it is liable to the alternatives of all other remedies which have a supposed relation with disease, the result of which is to be judged by experience; namely, first, it may not affect the disease, or change its character in any respect; secondly, it may change the state of disease; and the results of this change may be substituted disease, recovery, or death. The curative dependence of purgatives, mercury, and all those means, the sensible effects of which are those of derivation of fluids from seats of disease to unaffected ones, acknowledges the same conditions.

If the mere diminution of blood in local diseases would cure them, we are enabled by bleeding

to take away much more considerable quantities than may be derived by a seat of metastasis. In metastasis, the increased area of the vessels of a curative seat may perhaps not admit four ounces of blood more than their natural quantity; yet, trifling as the palpable increased derivation of blood is to the seat of secondary disease in some instances, the occurrence of such disease is found to be curative of the primary one; while perhaps sixty or eighty ounces of blood may be drawn from several vessels, without producing much, or perhaps any, diminution of symptoms, and certainly without any change in the character of the disease. This failure is obviously for the reason that, in metastasis, the relation is a curative one between a primary and a secondary disease; while in other derivations from a primary seat of disease, the force only of a concurring cause is diminished; the diseased condition is unchanged in the seat it occupies, and a curative relation is not manifested by any other. For the same reason, artificial irritation, or artificial seats of suppuration, fail of producing the curative results of spontaneous related ones. Thus, we may employ rubefacients, or vesicatories, or establish drains of any size by caustic issues; but so far from curing, they may perhaps increase an affection of the head, for example, which in similar cases is known to have been cured by an anthrax, or an abscess in the same situation.

Thus it appears, that the augmented volume of blood in local disease is dependent upon the continuance of that disease; and remedies which are

not curative of the disease will not cause it to cease by diminishing the quantity of blood which this part obtains: even if the vessels should be totally deprived of blood, (as by syncope, in inflammation of the conjunctiva) the disease is not cured by such deprivation of blood, and consequently can acknowledge no primary dependence upon it. It is therefore resumed, producing derivation of blood among its phenomena, as soon as the state of syncope has ceased.

The diminution of the quantity of blood in local disease is, as before observed, a reduction of the force of a concurring cause; and it is probable, the energy of the diseased state may, to a great extent, be diminished by a diminution of the material; as strength, or the general energy of life, is otherwise diminished by defective nutrition or starvation. And this, being a general dependence, may be allowed to furnish, with much limitation, something like a general principle of cure. But so long in general as the state of disease is preserved in a seat of preternatural derivation, it arises out of the necessary pathological relations of such disease, that there should be a compelled increase of fluids in its seat; and while such disease continues unsubverted, it will derive to its seat a relative preternatural quantity of blood under the most extreme depletion, as long as there is any blood to be had.

In connexion with the principles of blood-letting here suggested, it should be observed, that the local or relative quantity of blood is diminished proportionally to the diminution of the positive

quantity in the system, provided the power of derivation remains the same.

As, however, the term determination is a familiar one in medicine, and as a better, which will agree with every variety of the symptom, or rather with its different causes, does not present itself, there appears no objection to its continued use, provided errors of practice are not founded upon the presumed sufficiency of its explanations.

CHAPTER V.

ORIGIN OF DISEASE IN THE ABDOMINAL VISCERA.

It is a prevalent doctrine, and not always a pernicious one, (for the reason that the practice connected with it is better than the doctrine) that disorder of the stomach or liver is the cause of almost every other local disease. I have given elsewhere* a complete refutation of this doctrine, and I fear it will now be difficult to avoid repeating former discussions. Indeed, it would seem altogether superfluous to speak of the subject again in this place, if it did not appear to require here some notice, as forming a part of our present pathological examinations, in agreement with a general design.

If an organ, which has preserved a healthy state for many years, becomes diseased, under a continuance of the same influences, or the same habits, it must happen because the predisposition of the organ is changed.

Predisposition may be said to be of two kinds, or rather to have two stages: the first is owing to peculiar properties conferred on the ovum, which in time develop changes and symptoms, under influences not generally productive of disease; the second, as when certain habits are adopted, which do not at first produce disease, but which so far

* Indications which relate to the laws of the organic life.

modify the condition of health, that a predisposition is formed from the long continued reiteration of their influence, and a relation is opened with certain exciting causes which produce symptoms, in connexion with such predisposition, which would not otherwise occur. Instances of the first, are the secretion of the testes at the age of puberty, and the occurrence of hereditary disease, as of consumption at the age of two-and-twenty, &c. Examples of the second are, as when a person (then in health,) enters upon a business, which, at first, is not productive of disease; but in time, from reiteration of its influences, by progressive change or predisposition, occasions loss of appetite, nervous irritability, broken rest, loss of flesh, &c.; or, if a habit of living is adopted at any period of life, a progressive predisposition to disease may be traced to that period, which in time is declared by symptoms.

Now, if it can be proved that the abdominal viscera are the only ones that are liable to the natural predisposition or idiosyncrasy, or to be exposed to the influences which produce the acquired predisposition, then it may be conceded that to these organs belongs the exclusive privilege of originating disease. But, as disease is developed from one or other of these predispositions, so if other organs are liable to the idiosyncrasy by which disease might occur, even under the most favourable circumstances, or to the influence of external causes, which, by progressive disorder, come in time to produce symptoms, then the original seats of disease, so far from being limited to the abdo-

minal viscera, will have as wide a range as the whole system, in which one or other of these modes of originating disease may prevail.

But it appears that, so far from all other diseases originating in the stomach, the diseases of the stomach itself (so far as their history can be ascertained,) are not unfrequently produced by diseases which originate elsewhere. Thus, intense study will produce dyspepsia—the anxieties incident to many occupations are among the most frequent causes of it; the continued prevalence of any one passion will occasion it. The primary relation of these causes, it will not be disputed, is with the brain. They operate through the medium of the senses, and the processes are continued and recognised in the state of mental disturbance, which disorders the stomach; and the disease in this latter organ, coming to have the force of a reagent, produces in turn the phenomena of disordered digestive organs, and those incident to their sympathetic connexions.

Thus also, disorder of the stomach might originate in the skin, as in such a case as the following.—A flannel waistcoat was suddenly left off at the commencement of summer, in warm weather: the weather immediately after became cold, and the perspiration, which had been copious, ceased from the time of leaving off the flannel waistcoat. Pain in the head, loss of appetite, white tongue, irregular bowels, succeeded in the form of chronic disease; and although afterwards the weather became intensely hot, it was many weeks before perspiration was restored in any degree; during which time the

stomach was considerably disordered, and it recovered from this state only by a long course of treatment.

In short, we should perhaps be justified in the conclusion, that there is no part of the body in which disorder may not originate: and so far from its having been occasioned by, may be itself the cause of, the disordered state of the stomach. Thus calculus in the kidney will produce habitual furred tongue and impaired appetite; a fracture of the arm, a wound of the leg, a stone in the bladder, a disease of the prostate, a cancer in the face, a perpetual blister, &c. may all be productive of disorder of the digestive organs.

It is meant here to demonstrate only that the universal origin of disease in the digestive organs, as a pathological doctrine, is false. At the same time, it is highly probable, that the digestive organs are often primarily affected in disease, although the instances of this are not so numerous or so palpable as may at first sight be imagined.

Disorder of the stomach (and its consequences) is frequently in connexion with torpor, or irregular action of the bowels; which may be said to be an instance of the origin of disease in the digestive organs. How far this state of torpor may be dependent upon a disordered function of the nerves, distributed to the intestines; and how far the disorder in them, if admitted, may be dependent upon states of disease of their branches, or originating in their centres, are questions, which I presume, we want facts to determine. In this deficiency of facts, we may (setting conjectures

of possibilities aside) rest upon our experience; and, assuming nothing beyond it, it may be allowed that these, and many instances of the same kind, (as when disease appears to commence in the liver, rectum, &c.) are examples of an origin of disease in the digestive organs. There is no reason why the stomach itself should not also be the first sometimes to assume a state of disease. We can scarcely insist upon this privilege with respect to all other parts, and deny it to the stomach, whose organization disposes it equally to disease, and whose offices would seem to render it peculiarly liable to this state.

Indeed, the opinion of a general origin of disease in the stomach has obtained its chief credit from a consideration of the offices of this viscus. It is the fashion to quote the artificial and depraved habits of mankind, with respect to food; I am not however aware that these habits are greatly different now to what they were three hundred years ago, or that they are carried to an equal excess, when, if we may judge from traditional and recorded traits of physical character, disordered stomachs were less prevalent than they are now: the real reason of which, I believe, is, that disorder of the stomach commonly arises from disturbance of the function of the brain, which organ is now more exercised than formerly, owing to the greater prevalence of education; to habits of more general refinement, which increase sensibility; to the increased demand upon invention, and to the anxieties attendant upon the keen competitions which must become general, in proportion as population

grows exuberant. From these causes also, the intellectual and animal functions, in which the brain is so peculiarly interested, prevail over the organic; and in proportion as the former are strengthened by exercise, or habitual derivation, the latter are weakened by the antagonist consumption of energy.

Among all animals, appetite is the general criterion of the fitness of food. With occasional exceptions, an animal, although surrounded by variety, will choose only the food which is agreeable to his nature. The appetite of the carnivorous animals leads them to eat flesh; sometimes in much greater quantities than we should think judicious: yet it seems to agree very well with them. The herbivorous animals select by appetite the food which is best adapted to them, rejecting such as is poisonous or unwholesome, however contiguous it may be to that which is proper for them to eat. Is the sufficiency of this guide to be allowed in all animals, and denied to man? It is said that the appetite of man is artificial and perverted, and that it would induce him to eat things which he would reject, if his original appetite, or the instinctive appetite, were retained.

It is certain that men eat many things, with which they are furnished by the progress of the arts and of civilization, which they would not have preferred in a state of savagism, and on which, in this state, they would certainly have no opportunity of exercising their choice. But, in a general way, with some exceptions, it is perhaps true, that the reason why a man in a state of savagism (in which state it may be said the correctness of his ap-

petite, because it is instinctive, is to be trusted,) feeds differently from man in a state of civilization, is, not that his appetite would not incline him to many made dishes, but that they are not offered to him. If the food adopted by habits of civilization is unnatural, how comes it that it is not rejected at first, at least, by children, whose appetites cannot well be vitiated by an education or practice which is new to them? The instinctive, or natural appetite of children, has been strongly insisted upon. What is it, they say, that instructs a child to find and to suck the nipple of its mother? Certainly a natural appetite: and what induces the same child to suck the first meat that is offered him, however artificially prepared, or perhaps to drink with avidity the first drop of wine or fermented liquor, and to show a fondness for it, as often as a recollection of former gratification is renewed by the sight of a glass? Still appetite: which should be natural, as long as it has not been practised in habits which are said to be unnatural.

Although I think it likely that too much stress has been laid upon the quantity and quality of the food which men take in this period of civilization, as an original cause of disease, I am perfectly willing to admit that habits may be formed by degrees, which in time grow into excesses which are pernicious, and at which the appetite of the individual would have revolted before it had become sophisticated by a gradual practice. But, if it be said that the feeding of men is unnatural, it must be observed also, that their habits and character in every other respect are equally artificial;

that there is a general harmony in the operations of nature, and that if appetite digresses from its original propensities, it is most probably in agreement with a corresponding deviation from the natural state in other respects. The distinction between nature and art is, in fact, merely one of convenience; the productions of art are, through the instrumentality of one of her forms, the productions of nature: but the former constitute a class which, being the result of causes which are not recognised in other departments, is separated and distinguished by another appellation. The feeding on pulse, upon which a slothful, unbrained Hottentot would get fat, would not support the energies of a statesman, of a player, or a soldier engaged in the arduous duties of a campaign. But if these men require stimulation, it will be said, it is no argument in favour of the alderman, who eats and drinks twice as much as the worthies just hinted at, and may possibly be as slothful, and, in a few rare examples, as unbrained as the Hottentot; by which I am far from meaning any disparagement; for perhaps the Hottentot is a much happier man than the philosopher; and happiness being the end, should also be the measure, of wisdom.

But nature, taking into the account the strange materials of which she has compounded man, and calculating that these materials would make his course in all respects a little eccentric, has provided against his sustaining so much harm as one would expect from the working of the various leavens which she has jumbled together in him.

If appetite, which is one disposition by which it will be allowed, to a certain extent, propriety of feeding is ensured, should, as they say, become depraved, or if it should be a little erroneous, there are two provisions in the stomach (and they are not peculiar to this organ,) by which disease, from ill judged selections of food, is averted or moderated. The first is a power of re-action, as it is called, by which the stomach is capable of rejecting any thing which is offensive to it; and the second is the force of habit, which reconciles it to bear things which it does not think it worth while positively to reject. These securities against disease, from improper habits of living, men trust, without knowing in what their security consists; and the infinite varieties and modes of life, which are found compatible with health, seem to show that the confidence in these provisions is not unworthily reposed.

To what extent disorders of the stomach may be imputed to prevalent habits of feeding, I am unable to determine; but I suspect, very generally, the disorders which are imputed to this cause have their origins rather in habits, or idiosyncracies, affecting perhaps primarily other seats, than in the quantity or quality of the ingesta; which, if disease is traceable to them, seem rather to act upon a predisposition with the force of an exciting cause, than to engross the entire responsibility.

But, supposing it certain in general, as it may be in some instances, that the quality or quantity of the food may disorder the stomach, there is no other part which is not also liable to an origin of

disease, independently of that of the stomach, from the same cause. If the stomach is disordered by food of an improper nature, the bowels become exposed to its action in their turn; then the function of the mesenteric absorbents; and then the whole system, through the medium of the circulation.

Thus, from this very cause, which makes its first impression on the stomach, the *origin* of disease may be in a distant part; and will be so, if the distant part is in a state of predisposition, which does not obtain in the stomach; and supposing such distant part to be the brain, the stomach, so far from originating disease, may become secondarily disordered by the operation of a cause, whose first relation is with itself.

The supposed disorder of the digestive organs has grown into a doctrine of general pathology, chiefly from observing that these organs are more or less interested in most diseases. But it has happened here, as in many other instances, that connexions have not been sufficiently scrutinized: and the consequence is, that they obtain the credit of causes. Nothing is more easy than to generalize; but this propensity is most commonly indulged by loose reasoners: the difficulty, in matters of science, is not to generalize, but to make just distinctions. Taking leave of this erroneous pathology, we will endeavour to state the true relations of the digestive organs in disease.

Disorders of the digestive organs may be classed as, I. exclusive; and, II. related.

The first class neither requires nor admits any subdivisions. The subdivisions of the second are:

1. Disorder originating in the digestive organs, and producing disease elsewhere, by simple extension.

2. Disorder originating in the digestive organs, and ceasing by the occurrence of disease elsewhere: which exemplifies a curative relation of other seats, in respect to the digestive organs.

3. Disorder of the digestive organs, originating elsewhere, and exemplifying simple extension of disease.

4. Disorder of the digestive organs, originating elsewhere, and holding a curative relation with respect to its primary seat.

5. Disorder of the digestive organs, of synchronous origin with disorder elsewhere; at least disorder of these and other seats, where the succession cannot be defined.

6. Disorder originating in the digestive organs, and producing disease elsewhere, which is neither one of simple extension, nor curative; but in which the secondary re-acts, and increases the primary disorder.

7. Disorder of the digestive organs, originating elsewhere, which is neither one of simple extension, nor curative, but in which the primary disease is exacerbated by that of the digestive organs.

8. Disorder of primary or secondary seats, including the digestive organs, with, or without curative relation, extended to other seats, also with, or without curative relation.

I. Exclusive disorder of the digestive organs is not very common. I cannot remember to have met with more than three or four cases, in which

disorder in these seats appeared totally unconnected with other disease. I have known several instances of persons, who have for many years had disorder of the digestive organs, which has been a source only of occasional inconvenience in other seats; but never above three or four, of persons who for many years have constantly had very furred tongues, costive bowels, capricious or impaired appetite, and yet have been well nourished, strong, apparently in perfect health, and rarely complaining even of uneasy sensations in any other parts.

II. Related disorder of the digestive organs is manifested more or less in almost every case, as will appear from the exemplification of its divisions.

1. Disorder originating in the digestive organs, producing disease elsewhere, by simple extension. To this class belong all those local diseases, which are the consequences of primary disorder of the digestive organs; such as some forms of chronic fever, occasional erysipelas, ulcers which occur in succession to disorder of the digestive organs, and with which unmitigated disorder of these organs is afterwards contemporary.

2. Disorder originating in the digestive organs, ceasing under the occurrence of disease in another seat. Cases of this kind are not very common. I have known consumption preceded by severe chronic dyspepsia (which, I believe, is more infrequent than is generally supposed), the symptoms of which have ceased in the course of the secondary disease. I have also known chronic

dyspepsia, which appeared for years to be of the exclusive sort, cease under a cutaneous eruption.

3. Disorders of the digestive organs originating elsewhere, and exemplifying simple extension of disease, form a very numerous class. To this division belong perhaps the largest proportion of those cases, in which nervous disorder is in connexion with disorder of the digestive organs. This combination of disorder generally originates in habits whose primary relation is with the mind, or the passions; such as habits of business, of study, or of amusements of the anxious and feverish sorts, as those incident to a life of dissipation, the taste for which is sometimes gratified without frequent gastric debauches. If a girl is in love, the disturbance of the brain, by the excitement of a passion, may give her a white tongue, loss of appetite, &c.; perhaps also a vicarious determination to the head or chest, connected with irregular menstruation, producing hysteria, or even alienation of mind, or spitting of blood, and perhaps eventually consumption. Such determination to the head, &c. with various shades of modification, are the more likely to occur, if the progress of the amour is marked by systematic opposition, cross accidents, regrets, anxieties; by the alternations of fears, hopes, mortifications, revived expectations, joy, disappointment, &c. These forms of disorder are generally those of simple extension: the function of the brain is first disturbed, the stomach becomes disordered in consequence; and the disturbed state

of the brain, expressing itself with more or less severity in the several instances, continues without mitigation from the secondary affection of the stomach, by which it is more frequently increased than diminished.

4. Disorders of the digestive organs, originating elsewhere, and holding a curative relation with the original seat, are of frequent occurrence. Thus, fevers abate upon the supervention of diarrhœa, and more commonly upon the appearance of stools, which prove that the state of disease is assumed by the abdominal organs of secretion. In such cases of fever, it is common for the symptoms to continue without mitigation; and a daily purgative brings off only watery stools, not unhealthy in point of colour. These alvine discharges, perhaps spontaneously, or under the repetition of habitual purgatives, become black, or dark, fœtid, and consisting of diseased secretions: from this time the severity of the general diathesis abates, and the patient becomes speedily convalescent. In chronic disease also, the character of which is not defined, such disease ceases when the abdominal secerning system assumes an unhealthy action, displaying itself by diarrhœa, or stools which consist chiefly of depraved secretions. It is possible that one or more abdominal organ of secretion might in some instances be a seat of disease, which produces or maintains the general diathesis, and that the mode of cure here alluded to is the common one, of an ending in secretion. In chronic cases this connexion is rarely unmixed: such a termination of disease is generally in con-

nexion with previous disorder of the digestive organs, affecting chiefly the stomach. I cannot remember more than one case, in which chronic disorder, displaying itself by general irritability, frequent muscular pains, of short duration, watchfulness, &c. was accompanied with a clean tongue, good appetite, regular and healthy stools; and terminated in a prodigious discharge of black feculent matter from the bowels. I have also known alvine discharges of this depraved character to occur in chronic disease, the symptoms of which have nevertheless continued, without the slightest apparent change; in the instance now alluded to, copious and black evacuations from the bowels continued four or five days.

5. Synchronous disorder of the digestive organs, with that in other seats, is often met with, both in acute and chronic disease. Thus, in pneumonia, acute rheumatism, phrenitis, &c. white tongue, loss of appetite, accelerated pulse, heat of skin, and local pain in the chest, muscles, or head, all succeed, after the rigor, so perfectly together, that it is impossible to say, with any regularity, which affection of seat is first indicated by symptoms. In chronic diseases also, as from unhealthy occupations, bad air, &c. a person gets generally out of health, with perhaps no defined disease; and from the beginning, disorder of the digestive organs only seems to be a part of a more general affection, in the origin of which the stomach, the brain, perhaps the offices of respiration, and of the skin, were all so mutually and synchronously engaged, that it would be difficult to say which

part, if either, was first affected, or the affection of which was more or less important in regard to the rest.

6. Disorder originating in the digestive organs, and producing disease, neither of simple extension, nor curative, but one which increases the primary disease, is not of rare occurrence. Thus I have known a violent erisypelas of the hand and arm, which has been exquisitely painful, and attended with a prodigious chronic eruption both lymphatic and purulent, which has succeeded to long continued disorder of the digestive organs, indicated by furred tongue, brackish taste in the mouth, impaired appetite, irregular bowels, and general debility. The secondary local disease in this case, augmented throughout a chronic course, the symptoms of disorder of the digestive organs; and the subject had eventually in connexion with it typhous fever. The same relation is exemplified in some irritable ulcers, which first occur from disorder of the digestive organs, and then acting rather by sympathetic irritation, than by substituted disease, increase the symptoms in the primary seat.

7. Secondary disorder of the digestive organs is not unfrequently not curative, but it re-acts upon, or increases the primary disease. This state of the digestive organs will maintain disorder of the head, and nervous disorder, when the subject has long been removed from those habits or influences which first disturbed the function of the brain, and through this medium that of the stomach. Thus also, local injuries first disorder the stomach,

and this state of the stomach maintains a disordered state of constitution, in which the local injury participates. Or, as it sometimes happens, a local injury, as a gunshot wound, which has become chronic, not producing for a time disorder of the stomach, the health suffers but little in consequence; but if from any cause, as from confined matter, or exfoliation, the irritation of the wound disorders the stomach, the disorder of this latter viscus remains, impairs the general health, and acts prejudicially upon the local injury, after the cause which excited in it a temporary exacerbation has been removed.

8. Instances of disorder of primary or secondary seats, including disorder of the digestive organs, with or without curative relation, extended to other seats also, with or without curative relations, are met with in the more complicated forms of disease. Thus, disorder of the head produces disorder of the stomach; disorder of the stomach, abscess, boils, cutaneous eruption, or diabetes: the first may cease upon the occurrence of the second, or if it does not cease altogether, which is seldom or ever the case, it may lose certain symptoms, as those of opening and shutting, acute burning or pricking pain in one spot; the second may cease, or be mitigated, upon the occurrence of the third; or they may all three exist together, or either one may exacerbate the other.

The most prevalent form of dyspepsia which has occurred in my experience, is that denoted by furred tongue; brackish, or bitter taste in the mouth; irregular appetite, which is sometimes

good, but more frequently otherwise; flatulence; torpid bowels; nervous irritability; disturbed sleep, which is sometimes morbidly profound; general uneasy sensation, perhaps with or without pain in the stomach or bowels; occasional depression of spirits; frequent languor; sudden changes of temperature of the skin; cold feet; and most commonly, a tendency to disorder of the head. The degrees and combinations of these symptoms afford the principal diversities which I have observed in dyspepsia.

The theory of the disease supposes that the torpid state of the bowels is the cause of the other symptoms, and that defective secretion of bile is the cause of the torpid state of the bowels. In this place, it is necessary to observe only, that these explanations are more easily assumed than proved.

This form of disorder is commonly treated by diet and medicine. The plan of diet consists in refraining from all food which may be supposed to be difficult of digestion; in moderation in its quantity; in abstinence from malt liquors, and all those things which are supposed to be exciting causes. The medicines consist generally of aperients; perhaps of small doses of ipecacuanha, bitter and aromatic infusions, &c.

With respect to the usual treatment of dyspepsia, by avoiding all those articles of food which serve to increase the symptoms, I have known many, perhaps some hundreds of cases, so treated, but never knew one cured by it. The disease may be thus palliated; but I doubt if the disposition to it is not increased. There are three errors in this

careful and limited selection of the articles of food. The first is, that the stomach can often digest with ease things which people have determined *à priori* it ought not to digest at all. The second is, that by not keeping the digestive function adequately exercised, it is apt to become capricious, and either loses the inclination or the power to perform its duty. The third is, that whereas the bile is supposed to be the natural stimulus to the action of the bowels, I have reason to think that the food is not a little concerned in enforcing this action: that people may have griping pains, diarrhœa, tenesmus, &c., with white stools, in which it is presumed there is no bile; that if persons eat sparingly, the bowels will not act regularly, for want of adequate stimulus derived from this source; that if they eat abundantly, the stimulus of the ingesta will, to a great extent, contribute to, or produce regularity of the alvine evacuations.

I have known a change of diet, from a starving one to one of repletion, followed by regularity of the bowels, so that under the latter regimen the subject did not take an aperient medicine for two years: and under the former, he scarcely ever had an evacuation which was not procured by medicine. I knew a young man, twenty-one years of age, who had dyspepsia; he was, however, in other respects, healthy and vigorous; he had a soft pulse, commonly of sixty, and was able to walk ten or fifteen miles in a day without fatigue. His bowels were very torpid, and he was highly nervous. He had been directed to eat only

food of a certain quality, and of this food, which was weighed, to take only a few ounces; to drink water, and to procure stools by large doses of the compound camboge pill. I recommended him to leave off his pills, eat as much as he could, and of what he liked, and to drink a quart of strong beer every day. He was persuaded to enter, with great timidity, upon this plan; and in three or four days, his trouble was not that his bowels would not act, but that they acted too much, or too frequently. He was frightened at this effect, and returned to his old regimen, which he persevered in; and in a few months he died of an atrophy: my construction of which event was, that he was starved to death; notwithstanding he possessed a good appetite, and the means of procuring as much food as he liked. It is to be remarked that there was not, in this case, any evidence of diseased mesenteric glands.

Although, in dyspepsia, the bowels may not act with regularity under any plan of diet, I believe their action will approach nearest to regularity under the regimen of repletion, that is, of eating and drinking abundantly: this plan will produce occasional inconvenience; but it is attended, so far as my observation goes, with a better general state of health than when the opposite one is adopted. It will every now and then be productive of an exacerbation of the symptoms; and then a purgative, or a short course of purgatives, with a change of diet will at once be a remedy to the exacerberation, and have the effect of producing

afterwards a comparative freedom from the complaint, and an improved state of health. This improvement will, perhaps, last many months; during which time the less the patient thinks about his stomach and bowels the better. Nature might take the matter into her own hands; and if the bowels are neglected, she will, perhaps, give the patient a slight attack of cholera, which will be followed by all the improvement which usually succeeds to a trifling crisis of a disorder. The benefits of an occasional debauch have been remarked by physicians in almost every age; by which is meant that getting so drunk as to vomit at the time, and feel very uncomfortable for a day or two afterwards, will sometimes avert other disease, and supersede the use of medicine. This remedy, however, as less objectionable ones may be had, I am far from recommending.

Disordered states of the stomach, I know not with what strict propriety, are said to be either torpid or irritable; the torpid state is accompanied with furred tongue, &c., the irritable with a red and clean one. I have reason to think the distinction just (though the implication of the terms may be objected to), and that the two states may require a different plan of regimen: my experience, however, is chiefly of dyspepsia with a furred tongue, and my remarks are applicable only to the combination of symptoms first described. At the same time the phenomena of every form of dyspepsia are so mixed, and the symptoms said to characterise one, so often exhibited by the other, that I

doubt if any regular pathological difference can be assigned, or whether any distinct rules of treatment may be founded upon what appear to be only varieties of the same disease.

There are many diseases which we treat with advantage, upon something like a principle, derived from the effects of habit. I believe dyspepsia is, to a certain extent, one of them. If the eye is so irritable that it will not bear the light, we make it bear laudanum, or æther, and afterwards it does not regard the lesser stimulus of light. A similar illustration is afforded by the effects of muscular exercise: a person accustomed to walk fifteen miles a day, would think but little of walking five. So with respect to the stomach; if it will not bear moderate stimuli, or seems unequal to moderate exercise, we must make it familiar with immoderate stimuli, and harder labour, and it will then cease to regard the inconvenience imposed by its habitual duties. The principle is illustrated also by the frequent use of aperient medicines: if the bowels are accustomed to the preternatural stimulus of medicine, they will not act under the natural one of food. A practice on this principle has been greatly insisted upon by Sir George Gibbes of this city; and I think his views on the subject, to a great extent, correct in theory; and the benefits of the practice, in appropriate cases, appear fully confirmed by experience.

Of the treatment of dyspepsia by medicines, the aperient ones, with perhaps bitters, usually resorted to, are the only ones from which advantage is in general derived. This benefit consists in mode-

rating the symptoms; at the same time the exacerbations spoken of, as arising from the neglect of aperient medicines, are, by the use of them, averted. Whether this is an advantage may, perhaps, be questioned; the copious evacuations, or temporary diarrhœa, to which the dyspeptic who neglect their bowels are subject, produce the most sensible relief, and melioration of symptoms afterwards. Perspiration is beneficial in dyspepsia, the symptoms of which are also relieved naturally by the copious secretion of urine, incident to nervous disorder, so frequently in connexion with dyspepsia.

Blue pill, so far as I have had opportunities of observing its effects in dyspepsia, and these opportunities have been very extensive, appears, in doses of four or five grains every night, at bedtime, to increase the symptoms; but they are most commonly improved in a short time after this medicine has been discontinued. In lesser doses, as of one or two grains every night, continued for a longer time, it has produced no perceptible increase of the symptoms, and is often followed by a more healthy performance of the abdominal functions; which improvement, as the change induced in the action of these organs is gradual, and sometimes complete, is occasionally permanent; or at least continues for some months. By this medicine, in very small doses, the symptoms may sometimes be made to cease under its use; and the disposition which produces them may also be subverted, if the remedy is continued for a sufficient length of time. The former effect, even

that of diminishing symptoms, is, however, by no means regular, from blue pill, in any doses: and the latter, that of indisposing organs to a recurrence of the symptoms, is a felicity of treatment which we can seldom boast. Mercury in all its forms is generally prejudicial in the dyspepsia, accompanied by excessive nervous irritability, without evidence of disordered biliary secretion: but even in such cases, an improved state of health, at a more or less distant interval, will generally succeed to its discontinuance, provided it has not been carried to an injurious excess.

I have known the symptoms of dyspepsia improved during a treatment by ipecacuanha, as recommended by Daubenton, in a dose of one grain every morning before breakfast. Although in some cases this practice has been pursued with regularity during three or four months, I have never known dyspepsia *cured* by it: some benefit only has been obtained during the course which has continued a short time after it. The torpid state of the bowels has been in some cases increased; and in others, the evacuations have become more easy and regular under a treatment by ipecacuanha. A combination of sulphate of zinc, in doses of a grain, twice or three times a day, with rhubarb, and extract of gentian, is sometimes useful in dyspepsia. I have known an obstinate spasmodic asthma, which appeared to be intimately connected with disorder of the stomach, cured (or cease) by a perseverance in this medicine for some months.

Sea-sickness, which at first produces costiveness,

is sometimes followed by regularity in the alvine discharges. When persons have suffered considerably from sea-sickness, continued for a week or ten days, the bowels will sometimes act afterwards so as to produce regular and copious evacuations. At the same time, the stomach has not, in persons of weak constitution, readily recovered from the impression made upon it by this influence; the appetite has been more impaired, the tongue more furred, and the brackish taste in the mouth increased, for some months after the sickness produced by this cause; during which period the stools have been copious, regular, and healthy. I have been informed that the symptoms of dyspepsia frequently cease in the course of a long voyage, and do not recur except by the renewed influence of the exciting causes.

The symptoms of dyspepsia are sometimes improved, and more frequently increased; or improved in some respects, and increased in others, under a course of purgatives; and if the purging is of an active kind, the torpor of the bowels is afterwards greater than before. The state of the stomach is not so rapidly improved after the discontinuance of these, as of some other remedies, and the general health is longer disordered: the nervous symptoms also, accompanying dyspepsia, are almost invariably increased by violent purgatives. These inconveniences are, however, rarely permanent; and when the effects of the remedies have completely passed away, an improved state of the digestive organs succeeds to the changes induced by these, as to those of most other preter-

natural agents. Purgative medicines are, in general, not successful in mere disorder of the stomach, accompanied only with nervous irritability, and perhaps headachs. But they are eminently successful if the disorder of the stomach is accompanied with disordered function of the liver, or chronic pain in the side or stomach. I have known a fixed pain in the stomach which had existed more than twenty years, and which, for several weeks in each year, suffered one or two exacerbations every day, which terminated in vomiting, perfectly cured, and never afterwards returned, by a course of efficient purgatives, continued for six weeks. As a treatment for affections of this kind, I have found a pill, containing a grain and a half of aloes, one grain of blue pill, and one ipecacuanha, taken three times a day, a good combination.

Purgatives, for the cure of any form of chronic disease, whether in the digestive organs, or in distant seats, frequently increase the symptoms in the early periods of their use. This effect is rather desirable than otherwise, inasmuch as it proves that the remedy *has a relation with the disease*, and gives some ground for hope, that a means which thus shows itself capable of a preternatural influence on a state of disease, may be capable also of subverting this state, if continued for a sufficient length of time. Such increase of symptoms, therefore, as indeed is commonly remarked, is rather an argument, on many occasions, for the continuance or even increase of the remedy, than for its suspension or diminution. I have rarely seen

any permanently ill effects from purging; indeed I never met with more than one case of what is termed hypercatharsis. A woman, the subject of this case, underwent a long course of purgatives for disorder of the head: the head was cured; but by a substituted disease of the bowels; which was afterwards permanent, and occasioned frequent purging and griping stools, accompanied with blood, and sometimes with cylindrical portions of tough lymph in large quantities. It was at this time that I first became acquainted with the case, the symptoms of which improved, or nearly ceased, under a long and tedious course of treatment, in which small doses of blue pill, as one grain, combined with three of rhubarb, and one of ipecacuanha, taken every night for three or four months, appeared the most advantageous after the total failure of every other means. It should be added, that the drain of an issue was also in this case established in the leg, at about the period when the symptoms began to improve.

I have known the symptoms of severe dyspepsia totally cease under the influence of a caustic issue, employed for the cure of another local disease. The tongue, which was before white, has become clean, the bowels regular, and the appetite greater than it had been for many years. Adhesive plasters, containing six or eight grains of emetic tartar, applied over the stomach, which bring out pustules in two or three days, are sometimes beneficial. A seton in the side, for a supposed disease of the liver, has increased the symptoms of dyspepsia as long as it has been continued, which was about

six weeks. I have reason to think caustic issues in the side, in cases of diseased liver, more effectual, and certainly less irritating, than setons.

I have many times remarked that the symptoms of indigestion are improved or cease under the influence of an issue, which has become an unirritating drain. All the means of artificial suppuration increase commonly the symptoms of dyspepsia in the first stage, when they are sources of irritation; but I have reason to think that not only the symptoms of very severe dyspepsia may cease under the long continued operation of an issue, but that a suppurative process is among the most efficient means we possess of *subverting the disposition* to this and certain other diseases, and consequently of preventing their recurrence. This result depends, first, upon the circumstance whether the seat of the issue becomes a related one with that of the disease; and second, upon the nature of this relation, which, like that of all other preternatural agents, is liable to the results of mitigated disease, increased disease, substituted permanent disease, or progressive disease, terminating in recovery or death. The latter event would, however, be rarely hazarded from the continuance of an agency which was obviously prejudicial, and the control of which is in our power. The benefits of issues have appeared the most conspicuous in persons of a scrophulous habit, or in those who were disposed to diseases of the skin.

The connexion between dyspepsia and diseases of the skin has been before remarked. Those who have written on diseases of the skin, with a

great appearance of learning and connoisseurship, have done little more than multiply unnecessary and trivial distinctions, and propose a jargon of barbarous terms, which none but persons of very corrupt taste will take the trouble to remember. I do not, in the works alluded to, remember to have met with any thing like a principle of the pathology or treatment of these diseases. In this place it is necessary only to remark that, as we have seen in our analysis of the relations of disorder of the digestive organs, diseases of the skin are variously connected with such disorder; so a long continued treatment, by small doses of blue pill, perhaps with colocynth, or aloes and ipecacuanha, will cure many of them, without recurring to the more powerful agency of calomel, corrosive sublimate, or arsenic: and, as a local application, hartshorn and water, in the proportion of a dram of the former to an ounce of the latter, is almost a specific in many chronic diseases of the skin, attended either with lymphatic or pustular eruptions, whether confined to one spot, or extending over a whole limb. The effect of this stimulus is to exchange a peculiar for a common inflammation; and, I presume, by the same mode, liquor ammoniæ, liquor potassæ, and turpentine will cure tinea capitis: the proportion of hartshorn is to be regulated by the irritability of the surface, and either applied constantly or occasionally, according to circumstances. This remedy has long been employed in erysipelas, and I have used it with great success in almost every case of disease of the skin described above, which has fallen under this treatment: if continued after

the specific character of the disease seems to be subdued, it appears to irritate and produce troublesome exfoliations of the cuticle.

I am led to believe, from my own experience, that there are very few diseases of the skin which may not be cured; I do not remember ever to have met with any. The cure of diseases of the skin is accomplished on the general principle of subverting a diseased action; and this may be done, (1.) by external applications to the seat of the disease; (2.) by internal remedies, which act chiefly on the skin, and on this part, most probably, by an influence on its circulation; and (3.) by internal remedies, which cure disease of the skin by their action upon related seats; as the stomach, bowels, kidnies, &c. The most effectual of the external applications are sulphur, tar, the different forms of mercurial ointment, hartshorn, zinc, acetate of lead, spirit of turpentine employed with oil as a liniment, which I have found successful in some inveterate diseases of the scalp, &c. All these remedies tend obviously to produce what is called a new action. The internal ones, which act chiefly on the skin, are sulphur, arsenic, ammonia: these produce heat of skin, temporary fever; and thus substitute an artificial for a natural diseased action. The remedies which cure diseases of the skin by their action upon related seats, and may be regarded as means of revulsion, are purgatives, emetic tartar, calomel, corrosive sublimate, nitre, &c. These means may be variously combined; and any of them which are capable of changing the character of a cutaneous disease, if employed in an efficient manner, for a sufficient length of time, will

generally cure it. It may be asserted that sulphur, antimony, mercury, and arsenic are adequate to the cure of almost any cutaneous disease: but to accomplish this object, the mode of employing these agents must in some instances be so efficient, that it is frequently more judicious to suffer the disease of the skin to continue.

If, in dyspepsia, it should be found that tolerable health and comfort are obtained, upon the easy terms of procuring regular evacuations from the bowels, this may be attempted either by diet or medicines. The bowels act more regularly under a full and indiscriminate diet than under a spare and select one. Of all the plans of obtaining regular alvine evacuations by food, I know of none so effectual as a residence in another country; as in France, where the mode of preparing food is so different from that to which Englishmen have been accustomed. As dyspepsia is also much more prevalent in some districts than in others, so it is probable, that either the atmosphere or some other local circumstances, may have some influence on this disorder. But the chief advantage of a residence on the continent is derived from the habitual or rather free use of the French wines, which I believe, from ample testimony, are exceedingly conducive to the health of the dyspeptic. A regulation of the bowels by some such means is very desirable in cases of habitual costiveness, accompanied by a state of the rectum which will not bear purgatives of any kind. It is, however, probable that the tendency to disordered action may again prevail in many such cases, when the change has become habitual.

A principal object of regularity in the action of the bowels is, that the excrementitious part of the food should be regularly discharged. If ordinary food is not a sufficient stimulus, owing to diminished irritability of the bowels, it would be desirable to compensate for the want of irritability of the bowels by the addition of aperient quality to the food. This is done by the wines above spoken of, which are every day mixed with the principal meal. I have somewhere seen, that castor oil was first introduced as a medicine from an experience of its aperient quality when taken habitually with salads. A gentleman once told me that he had no occasion to take aperient medicine for twenty years: when he thought he wanted physic, he drank with his dinner a bottle of claret, in addition to his usual allowance of other wine. This plan, however effectual and agreeable, is a precedent which it would not be convenient for all persons to follow; and, therefore, as well as on some other accounts, I would not be understood to give it an unqualified recommendation.

If the stimulus of food is found inadequate to produce regular discharges from the bowels, this effect may be secured, and the aperient quality of the medicine imparted to the food, by taking every day, during dinner, or towards the conclusion of this meal, two, three, or six grains of the compound extract of colocynth, or one, two, or three grains of aloes. With this view, it is desirable to ensure the ready solution of the pill, in order that it may mix with the food; and this object is promoted by the addition of a little soap, and perhaps sulphate

of potash, to each dose. A medicated aperient drink, or some fluid aperient added to the common drink, such as croton oil (of which, however, I have no experience,) would probably accomplish this view with more certainty. The quantity of the aperient will, of course, be regulated by its effect. When a gentle aperient is taken without intermission every day during dinner, it is sometimes necessary to diminish the dose rather than to increase it; and I have reason to think that a weaker aperient will keep the bowels regular if taken during dinner, than at any other time: the food and the medicine acting as auxiliaries to each other, a more adequate stimulation of the bowels is produced than when the stimulus of food and medicine act on the bowels in succession. Whatever may be the principle, I have found the practice (except in cases of peculiar obstinacy) generally successful.

It is a popular belief, that a habit of alvine evacuation may be established by an effort of this kind every day at the same hour. This practice is also a common medical recommendation. Although it may be founded, to a great extent, in truth, yet, as an unlimited direction, it may not be always advantageous. The effect of a periodical attempt at expelling the contents of the rectum is, in general, that a very small portion of feces is evacuated; the same is repeated the next day, and always without sense of relief; there is seldom felt a natural or pressing call of this kind; and feces continue to accumulate in the bowels until removed by a purgative or a spontaneous diarrhœa.

I am disposed, from experience, to believe that it would be better to make no attempt at an evacuation until the necessity of it is felt. The small portion of feces expelled from the rectum every day, if suffered to remain, would act as a stimulus to the bowels, and in place of a scanty and useless evacuation every day, there would be a copious and sufficient one once in two days, or even oftener, if no other efforts were made than those suggested by the impulse of nature. But persons might say, if they waited for a natural impulse, they should never have an evacuation. In general, they would be mistaken; and if they were not, their case requires that they should take an aperient pill every day, or one, two, or three teaspoonfuls of castor oil every night, or some habitual aperient. Many practitioners advise their patients to let their bowels alone; by which is meant, they had better not take medicine. This recommendation suggests a complete neglect of artificial management, or a trial of how far the dictates of nature may be entirely confided in with advantage.

The use of the rectum bougie sometimes disposes to regularity of alvine evacuations. This it does, merely by the stimulus of a foreign body in the rectum, acting as a suppository; an effect which would frequently be accomplished by the presence of feces, if they were allowed to accumulate.

The existence of spasmodic stricture of the rectum appears to indicate only a morbid irritability of the gut, not improbably owing to deficient habitual stimulation and distention, by the collec-

tion and passage of feces. Whether the stricture itself is a cause of any sympathetic diseases, I think may be justly doubted; and if the bougie which cures the stricture, cures also other accompanying symptoms, this success may be supposed with as much reason to be owing to the influence of the remedy upon the disease which produced the stricture, as to the removal of the stricture itself, which is, at most, but a symptom, and can have only the importance of a mechanical re-agent, by which the descent of feces into the rectum might be impeded. That the spasmodic stricture, so frequently discovered, opposes the passage of a bougie beyond it, is no proof that it would, unless very inveterate or accompanied with thickening of the structure, equally impede the passage of feces. A bougie is a foreign body, and is introduced into an irritable tube endowed with muscular power, in opposition to the order of muscular contraction. The muscular action of the gut is expulsive, and it very naturally contracts against the admission of a foreign body. This natural disposition is perhaps the cause of many supposed strictures in this part, which cease under the use of the bougie, because the structure, being accustomed to the stimulus, ceases to contract spasmodically upon its application.

It should have been mentioned, that considerable importance is by some attached to the simplicity of the food; as of eating but of one kind at the same time, &c. in order to agree with the digestive function. For my own part, this seems to me a very trivial matter; for it may be pre-

sumed that if fish occupies the inferior, beef the middle part of the stomach, and poultry or game its superior portion, these several articles would be as perfectly digested by the surfaces of the stomach to which they are opposed, as if either sort of food occupied the whole of this viscus. There are some other directions extant of similar quality, upon which it is not worth while to remark.

Cold affusion, or cold bathing, as mentioned by Dr. Hamilton, has an aperient influence on the bowels, and I have known this effect permanent, when the practice has been discontinued, after a long course of sea-bathing. I have heard the nitro-muriatic acid bath applied either as a pediluvium, or by sponging the body with the fluid, spoken favourably of by some, and others have reported it as altogether inert. A patient who, under other medical direction, had used it, as she informed me, with advantage, (that is, as an aperient) resumed it while under my care, but without any perceptible effect. In my limited experience of the remedy, it has appeared that the effect of the bathing is not increased by the medicated property of the water. This experience, however, has not been sufficiently extensive to sanction any positive opinion on this matter.

In conclusion, of the general pathology of the digestive organs, it might be said that they are a centre of very extensive sympathies; that disease might originate in them, and communicate disease to other organs; that disease might originate in other organs, and disorder those of the digestive functions; that disorder of the digestive organs

when primary, is sometimes spontaneously cured by the secondary disorder which it produces; and that the disorder of the digestive organs, when secondary, is sometimes curative of the primary disease. But that the disorder of these organs, whether natural or artificial, is liable to all the tendencies of related disease, of which that of the secondary is chiefly to be remarked; and the tendency of secondary disease may be to cure the primary disease, to co-exist with, or to increase it.

In the curative instances of secondary disease, it will perhaps be presumed that something like a metastasis takes place. This, however, is not certain; it is a relation between properties constituting the state of disease; and we cannot say that the secondary may not cure the primary, by communicated influence, as well as the primary produce the secondary disease in this way; both the modes of cure, direct and derivative, being probably exemplified by the operation of medicines.

CHAPTER VI.

ORIGIN OF DISEASE IN THE NERVES.

The general origin of disease in the nervous system is a doctrine which is at this time very extensively adopted. It cannot be said that this doctrine is now exclusively proposed by any individual; it has been more or less a received opinion since the time of Willis, and later advocates of it have only been distinguished by the slight modifications of the doctrine which they have suggested, and by the different degrees of zeal with which they have insisted upon it.

It is a proposition which is at least in agreement with our experience, and which, I believe, admits of logical proof, that the origin of disease is in the properties of life, or in those which distinguish the living from the dead state. But to say that the origin of disease is in the nerves, is to assert that life is either exclusively allied with these organs, or that these organs alone are concerned in the maintenance of life, and confer this principle, or these properties, on the other structures.

1. If the nerves are the exclusive seat of life, it follows, either that all the structures are modifications of nervous continuity; or else that there are structures which exemplify the phenomena of

the living state, and which do not run into decomposition, which is the chemical tendency of the structures, and which, nevertheless, do not possess life. The latter alternative need not be considered.

That some of the structures, which display nothing like nervous filaments, do nevertheless consist of a modification of nerves, has been by some conjectured. The muscles have been supposed to be a modification of nervous structure; but as the muscles have never been known to contain medulla; as there is no analogy in the appearance of nerves and muscles; and also as there is no analogy in their phenomena; that is, the nerves are never observed to contract on the application of a stimulus, although the muscles to which they are distributed may be made to contract by the application of such stimulus to the nerves; so there being neither analogy of structure, nor analogy of function, so far as we are capable of obtaining sensible evidence, there seems no reason for supposing that these structures are identical.

It has indeed been attempted to establish this identity of structure, from connexion of function. It has been observed, that muscular contraction, of some sorts, is dependent upon the continuity of nerves. The evidence of such a connexion amounts only to this; that the properties of nerves are related with the muscles, and act on them: but because properties of nerves can act on muscles, we have no more right to conclude that muscles are

nerves, than that the nerves are brandy, ether, caustic, or a scalpel, because these substances are capable of acting upon nerves. The action of one thing on another proves a relation only to the extent that it is witnessed: if it proved identity, men would be horses, waves would be ships, and the air would be all things.

Thus it appears that nerves are not identical with muscles: it is scarcely necessary to observe, that their analogy with bone, ligament, arteries, veins, and the parenchymatous substances, is still more remote: yet in all these, disease may originate.

Then if it appears false that the structures consist of a modification of nerves, it is next to be determined whether these organs, which are confessedly capable of acting on the other structures with which they are allied, are the organs by which life is produced and maintained; and whether by their function life is conferred on the other structures. Our first appeal must be to our experience.

2. We have no proof that the nerves possess any function which is independent of their centres, as the brain, spinal marrow, and perhaps the ganglia. In paralysis, the organic life is maintained, assimilation and nutrition proceed: if the nerves of a limb are divided, it loses only the capacities for sense and voluntary motion, which are the characteristics of animal life: its nutrition is continued, its secretions, absorptions, &c. are performed; and the wound will suppurate below the division of nerves. Inflammation and vesication

may also be excited in a paralytic limb. According then to our experience, the function of the nerves, of which we have no proof except in their connexion with their centres, is not necessary to organic life. If then, according to our experience, the organic life is proved to be independent of the nervous centres, and if the nerves, without connexion with their centres, have no function, how can it be said, that the formation, or maintenance, or phenomena of the organic life are dependent upon the nerves? In truth, there is no more reason for believing that the nerves are the organs by which the organic life is assimilated, and conferred on the other structures, than that the other structures produce life, and confer it upon the nerves.

But it is a fact, it will be observed, that although all the processes of organic life may continue to take place in a paralytic limb, or in one the nerves of which are divided, yet the nutrition of this limb seems, from an interrupted communication with a nervous centre, to be impaired, if we may infer this from the diminished size of such limb. That the bulk of a paralytic limb is in consequence of this state diminished, would happen very naturally from the circumstance of its not being capable of muscular exercise; or, in other words, there is less excitement in such a limb than in one the connexions of which are entire; and consequently a diminished derivation of blood from the participation of the derivative powers subservient to nutrition, in that reduction of energy which follows from the privation of a natural stimulus.

The evidence which we at present possess with respect to the function of the nerves amounts to no more than this: 1st, That an unimpaired connexion should subsist between those of a seat and their centre, or origin, in order that such seat should be capable of sensation or voluntary motion. 2d, That the nerves themselves have no relative function with the structures with which they are allied, independent of the influence which they convey from their centres. 3d, That although the processes of organic life are continued independently of the nerves, yet the privation of accustomed nervous influence, as a cause of excitement, which to some extent may be substituted by other stimuli, is attended by a diminished energy of some or all of the functions of organic life. 4th, That although organic life, consisting of a maintenance of the living principle, assimilation, and absorption of organic particles, may be continued without nervous influence, yet this influence, in some instances, may be necessary to the functions of organs; which functions are powers superadded to the general characteristics of organic life. Thus, the influence from a centre of nerves may be necessary to digestion, perhaps also to some glandular secretions, &c.

In those examples in which nervous influence either gives force to the common properties of organic life, or in which this influence is essential to a function, the properties derived from a nervous centre, in such cases, owe their efficacy to a relation which is between themselves and

other properties of life, which belong to the seat of their distribution, and which are assimilated in such seat. So that whenever the influence of nerves contributes to processes of organic life, it is by the communication of properties from a centre; which properties are common, and by no means constitute the function, which is made by the relation of the peculiar properties assimilated in the seat of such function, with common ones derived from the nerves. Thus the nerves distributed to the seats of voluntary motion, and of sensation, convey common properties to these seats; the phenomena of which result from a relation between these common properties, and those peculiar ones which belong to the seat, and which being independent of any other seat, are assimilated where their phenomena are observed, by their relation with blood, from which they select their elements; and reiterating this process as fast as they are consumed, thus maintain their own nature. Thus also the eighth pair of nerves convey common properties from the brain, which produce different phenomena in different seats, by meeting with the peculiar properties of these seats, and acting in relation with them: in their relation with the structure of the larynx they produce the phenomena of the voice; and in relation with that of the stomach, those of digestion. But if these properties, conveyed by the nerves from their centres, were the entire or identical ones of the function, they should confer alike a power of digestion and of speech on the larynx, and on every

membrane or muscle to which they are distributed, which power would be inefficient only for want of appropriate mechanism.

There seems, in general, no reason for assigning to the nervous influence an agency above that of a stimulus, by which is meant an influence which has a property in common with some or many other substances. Thus electricity will substitute, they say, the influence of nerves conveyed to the stomach, and conducive to digestion; and a mechanical stimulus of any kind will produce those contractions of muscles, below a division of nerves, which stimulus is otherwise supplied from a nervous centre, during the integrity of the organs. It is probable also, that certain other stimuli may have the same relation with the properties of the stomach, as obtains with respect to the nerves. But whether they have, or not, the independence of the life of the stomach, and of the peculiar properties assimilated in this seat, on the influence of a nervous centre, is in no degree invalidated.

This account seems to be merely a statement of the evidence. There are no proofs that any vital action or function is performed wholly by the properties of nerves, in connexion with their centres: the evidence proves only a relation between the properties of different seats, the phenomena of which seats result from this co-operation.

The evidence with respect to electricity is of much about the same kind. It is common to quote relations of electricity with life, as proofs of their identity: if all those things which are related with others were said to be identical with them, this

species of logic would very soon make a simple world of it.

We judge of the identity of things, not from the circumstance that one thing influences another, which in fact proves a distinction, which in old language would be expressed by the terms agent and patient, but by a comparison, either of the things themselves, related only with our senses, or if they are not cognizable to our senses, by a comparison of their phenomena. If their phenomena are perfectly alike, we conclude the substances to be the same: if their phenomena are alike in one or two respects, we conclude that the two substances contain one or two identical properties in common, as the same effects must always be produced by the same causes. If the phenomena of the two substances have a general resemblance, we conclude the substances to be nearly the same; but if the diversity of their phenomena, or the absence of like phenomena on the part of one of them, is the most conspicuous, we admit a resemblance in some respects, but at the same time infer a general diversity, corresponding with those general particulars, in which the resemblance fails.

Now with respect to the similitude of electricity to life. We observe that electricity is related with life, and acts upon it; this is no proof of identity. We observe also that electricity will substitute in some instances the properties derived from a nervous centre; in this respect there is an identical property common to it and life, which is also possessed by many other substances. We ob-

serve, also, that the formation of heat, and the faculty of generating electricity, belong to animals, and are dependent upon their life. The faculty of generating electricity, in animals, does not prove that electricity is even a constituent part of their life: it proves that it is a phenomenon of their life; but that it is a part of it, is no more to be concluded on this account, than that urine, or mucus, &c. is a part of life, because these are also products of it.

We have made out then only one point of resemblance between life and electricity, which is, that electricity will in some cases substitute a property otherwise derived from a nervous centre; which property, applied to the stomach, will aid digestion, in which respect, it has not yet been found that more common stimuli resemble it: applied to the voluntary muscles, it will produce their contraction, and in this respect the property is a common one to many other substances, which no one ever thought of identifying with life. But even the properties which are said to depend upon a nervous centre are not all of them substituted by electricity, which will stimulate muscular contraction, like many other substances, but like those substances also, it is incapable of conferring sensibility; or if electrical influence ever excites sensation in paralytic limbs, it is only because their sensibility is not totally extinct, and will therefore admit of sensation under the application of this, or of any other stimulus, of a powerful kind.

We have seen that electricity can do a very

little which is also done by life; there is then analogy in one property, but to be the same identity, there must be analogy in all; or to approach to such an identity, there must be, at least, a general analogy. The living principle maintains itself by assimilation from exposure to its elements; electricity is not capable of maintaining itself from its elements, but must be produced from them. Muscular power in the animal system is related with mind, and directed by volition; we have no evidence that mind, or volition, independently of the properties which distinguish the living state, can so ally itself with electricity. Animal life confers sensibility on structures; electricity can merely excite sensation, in common with chemical and mechanical stimuli. The organic life produces from a common material, arranges, and renovates, in the muscular system, the particles which compose muscle; in the tendons, those of tendon; in the membranes, those of membrane; in the bones, the constituents of these structures; and of all others, with all their circumstances, however diversified. Now if electricity were capable of doing all this, there would then be established only a general resemblance with life; analogies would afterwards be sought for, corresponding with those powers exhibited by the relation of properties of life in different seats, and more especially among the phenomena of disease. But until the pretensions of electricity to an identity with life shall be established by rather a more extensive analogy, it is superfluous to inquire how far the phenomena of electricity resemble those of dys-

pepsia, diarrhœa, consumption, abscess, or gout. If, perchance, electricity should be endowed with the properties engaged in these phenomena, it will be greatly indebted to its friends, for bestowing upon it attributes which it has never displayed. In the mean time, it is to be wished that experimentalists will go on multiplying their facts, and that they will abstain from reasoning upon them: they will not, however, err to any great extent in this way, if they will take the trouble to remember that so far as things are proved to be alike, they are alike; and where they are not proved to be alike, it is possible that they may be different.

The identity of life and electricity, or galvanism, has been inferred, as appears from the preceding account, from very slender premises: but the arguments just considered are among the best that have been proposed in favour of the sameness of the two principles, or substances, if they are substances. Among proofs of a looser kind, I have heard it said that electricity can be nothing less than life, because it is capable of counteracting the relation which naturally subsists between an acid and an alkali; so that these substances, however completely exposed to each other, should suffer no change from such exposure. Various other chemical feats are recorded of electricity, to say nothing about its connexion with magnetism: and as electricity is capable of decomposing chemical substances, and of subverting natural relations in this department, it is concluded that electricity must be life, or rather, as the latter is subordinate in rank, life must be electricity: that

as electricity performs certain chemical phenomena, so also it must accomplish those of the living state; a mode of reasoning which is much too liberal. It should, in justice to the cause of electricity, be mentioned also that this influence will make puppets, that is to say, the figures of men in pasteboard, or paper, dance: a property which claims for it a more intimate relationship with human nature, than some others, which have been cited in proof of this consanguinity.

Returning, then, from this digression, we have no reason for believing that the nerves have any function which is independent of their connexion with their centres. It appears that by the function of nerves, parts obtain sensibility, and a faculty of voluntary motion; but that such parts can live independently of the function of nerves: at the same time, although the organic life is independent in general of the function of nerves, the energy of this life is diminished by a privation of the stimulus conferred by nerves, in the habitual exercise of their function. Although organic life, consisting of assimilation, renovation of the textures, absorption, &c., is every where independent of nerves, yet where organs, in addition to these characteristics of organic life, have peculiar functions, as of digestion, perhaps in some cases also of glandular secretion, the properties of nerves are essential, as auxiliaries to these functions. I have formerly distinguished the life of seats as consisting of the assimilating, which is the characteristic organic life just described, the regular communicated, and the occasional commu-

nicated, properties of life. The first is the common life of every seat and structure, and maintains its own identity by a relation of affinity with its elements, which are contained in blood, and also separates from blood the organic particles comprising its seat, by an affinity subsisting between its vital properties, and those in alliance with such organic particles; secretion, absorption, &c., are accomplished by the life which is assimilated in respective seats, and which depends only upon its own existence, and a supply of its elements in a common material. The properties of the second, or of the regular communicated life, are those which belong to the assimilating life of another sphere, and are communicated to a distant one, unremittingly, in aid of an unremitting function, which is superadded to the common organic life: they are perhaps exemplified in respiration, and also where this influence is at present little more than supposed, that is, in the stomach, and in some glandular secretions. If these properties belonged to the assimilating life of the seat where their actions are displayed, or could assimilate with the vital principle of this seat, they would be independent of their distant source, and like the other properties of the organic life of such seat, they would be renewed with them from arterial blood. The occasional communicated properties are those which are assimilated in one seat, and are communicated to another, agreeably with the occasional purposes of the function to which they subserve, as the property of volition to certain voluntary muscles; or in disease, as when from disturbance of the life

of spheres, preternatural relations are opened; and properties assimilated in one seat are distributed to others, with which they before held no natural relation, and produce distant effects, dependent upon them, which arise from the establishment of preternatural relations *.

Thus, it appears that the function of the nerves, in health, is at most only a related one, with other properties, which, judging from their phenomena, are infinitely more numerous and important than those of the nerves in the seats to which they are communicated. It is probable that in the fœtus the function of the nerves is scarcely called into action; at all events, no effects of this function are manifested until the last four or five months of pregnancy: we know that the growth of the fœtus, all its peculiarities, its congenital diseases, if it has any, proceed, and are established, without nerves; which are not first formed, and then the other textures; but are synchronous in their growth with the other textures, or rather are observed to succeed some of them. These things happen from the growth, component relations, and continuity of life, which I have elsewhere called an organic spirit, which supports itself; whose properties are infinite, which properties are related with each other, and with those in contiguous and distant spheres; and by these relations they separate and combine; assimilate in one seat, are communicated to another; are derived from others,

* This division is elsewhere adopted, and its premises and connexions extensively considered. See Indications, &c.

agreeably with all the modes of causation; and according to these relations between vital properties, are the phenomena of the sensible kind, from the witnessing of which these properties are inferred.

It is sufficiently obvious, from this view, how gross that assumption must be, which attributes all diseases to a nervous origin. We have found the nervous influence to be little more than a stimulus in its general efficacy; and although the other properties of life are so numerous and so complicated, this stimulus forsooth is the only one which is liable to the modification of disease! Not to be prolix in a refutation of this doctrine, which is elsewhere as complete as language can express *, it may be remarked that inflammation and suppuration proceed in a wound below a division of nerves; as these are diseased actions in a seat where nervous influence is precluded, so it is presumed that disease might occur independently of this influence.

But it is not merely presumed that all diseases have a nervous origin, but it is said that all diseases have a local origin, the causes of which are either excess or reduction of nervous energy. This is a magnanimous conclusion, and it is spoken of as the doctrine of Broussais: how justly, it is scarcely worth while to inquire, though it may be observed, in passing, that there would be no great difficulty in showing that the doctrine is but a trifling, if any modification, of those which have for a long time prevailed.

* Indications, &c.

So far as experience enables us to remark the order of symptoms in acute disease, it does not appear true that disease always has a local origin. From perhaps exposure to cold, a person might have slight shiverings; these may be followed by flushes of heat, perhaps perspiration, and the pulse may be raised from seventy-four to eighty-six; the urine may be rather high coloured; the tongue not very white; the appetite diminished, with some degree of thirst; sleep interrupted; and with these symptoms, buffed blood. Precisely this connexion of symptoms I have often met with; the case has, perhaps, become chronic; and it is only in the course of it that the subject has complained of a local pain, which was, perhaps, on the right side; or, after the other symptoms, sense of tightness on the chest might have supervened, with or without cough, or perhaps a pain in either side, on inspiration. Where is the local origin of a form of disease characterised by such symptoms? I presume it would be very difficult to assign a local origin, in any one organ, for which reasons equally good might not be assigned in favour of any other.

In acute disease in general the precursory symptoms prove the establishment of a state of disease, before its particular seat is declared; and in general, also, the precursory symptoms of diseases are the same, whose seats are subsequently shown to be different. Thus, shivering, lassitude, slight fever, perhaps sneezing, may be the precursory symptoms of a common catarrh, of a quinsy, of bronchitis, of pleurisy, of inflammation of the liver, of phrenitis, of scarlet fever, of measles, of the small-pox, of

typhus, &c.; where is the *origin* of the abovementioned precursory symptoms? The organs which are afterwards chiefly affected become so in succession to a state of disease, which appears to have no one seat; and if it should be said a particular seat must have been first affected, although it did not appear so, should symptoms dependent upon local disease be the same, whatever organ or part indiscriminately the local disease might affect? It is by no means intended to deny the local origin of disease, which has been remarked time immemorial; of which our daily experience furnishes us examples; but it cannot be proved that a local origin of disease is universal, since such origin must be inferred from precedence of affection in one part; and it often happens, that the phenomena of disease affecting different parts, or the whole system, are synchronous.

Whether or not the universal, or even common origin of disease is in the nerves, has been already discussed, and there appears no reason why properties of life so trivial, in comparison with those of which no dependence on the nerves can be traced, should have the exclusive privilege either of originating, or of maintaining disease. This is one part of the doctrine which has been found to be false: that all diseases have a local origin is another part of the doctrine, which also appears to be false, or not proved: the third and last part of the doctrine is, that diseases not only originate in the nerves, and are local, but that all diseases are occasioned by excess or deficiency of nervous energy in their seats.

We can conceive of the distribution of nervous energy no extremes more perfect than those incident to paralysis on the one hand, and to the exercises of a gladiator on the other. In the one case, there is a total absence of nervous energy; and in the other, its vehemence cannot be exceeded. In the first case, life is maintained in the seat of paralysis without positive disease; and in the other, the most violent determination of almost the only nervous influence that is recognized is also not productive of disease, but to a certain extent promotes health.

But, it may be said, there is another property of nerves besides that of muscular power, to the deficiency or excess of which disease might be imputed, and this other property is sensibility. In the case of the paralytic limb, there is considerable or total loss of sensibility; which, if, in this extreme instance, not productive of positive disease, we need scarcely expect it to be so in its intermediate degrees; and with respect to increased local sensibility, it cannot be proved (although it is not here meant wholly to deny it) that the quantum of sensibility is ever increased; so far from its increase being a general cause of disease. Our experience respects sensation, not sensibility; and we find that sensation is according to the causes which excite it: there is always sensibility in seats sufficient for the strongest sensation, provided there is an adequate cause of excitation; thus, the amputation of a healthy limb would most probably be as painful as of one in a state of disease, or if not so painful, quite sufficiently so to show that there is

abundance of sensibility in parts, much more than is called into action in ordinary cases of disease, provided the exciting causes do not fail. Now the cases in which this excess of sensibility is supposed, are those where the sensation manifested has a history somewhat similar to our amputation; the sensibility of the part is excited by disease, and hence sensation. In this way sensation is excited by the external injuries which produce inflammation; and in this way, by the inflammations which occur spontaneously, and in which the sensibility of the seat is merely engaged, in common with other properties.

That this account is true, is further confirmed by the consideration that the nervous centres are, with respect to the seats of the distribution of the nerves, something in the circumstances of the heart, with respect to the distribution of the arteries; that these centres hold with respect to the nerves, or arteries, proceeding from them, only a common relation; and if one part does obtain an excess of blood in the one case, and of nervous influence in the other, it is not owing to the state of a centre, which has a *common* relation with one or more trunk, and their system of branches, but from a local state of disease, by which one part may obtain more blood, or more nervous influence, than another, although both should be supplied from common trunks.

The clearest example we have of a seeming power possessed by a nervous centre to direct its influence to a particular seat, is that of volition; which appears to originate in the brain, and is

capable of exciting the action of any particular muscle (the same power is more faintly exhibited in the senses of hearing, vision, &c.). But this instance is equivocal; for the properties which are transmitted from the brain, in volition, to any particular muscle, are conveyed by trunks which are common to other seats; and it therefore appears probable, that the reason why one seat of the distribution of a common nerve is peculiarly excited to action is, that the properties of this seat alone are so related with such a modification of the power of volition; precisely for the same reason as mercury rubbed into the thighs, and made to pervade the whole system, produces ulceration only of the gums. But as the brain can exercise, so far as we observe, no similar faculty with respect to the diffused sensibility (supposing this viscus, agreeably with a theory, to be a source of such diffusion) with which the textures are generally endued; that is, it can neither augment the sensibility, nor produce spontaneous sensation in any part, so, according to our experience, this power of specific determination is most evident in volition, or belongs at least only to the functions of animal life.

In the instances in which volition produces the action of certain muscles, it does not appear that muscular power is increased by any relation of the brain with such seat; but merely that the power which belongs to the structure is excited to action by the stimulus of volition; which property of excitation is common also to many other substances. That this apparent faculty of specific determination, if it be admitted at all, especially

belongs to the will, and more dubiously to the senses, is further proved by the exercise of the muscles supplied by nerves proceeding from the ganglia; which exercise is generally unremitting, and liable to violent exacerbation, expressed by spasm, &c. by the operation of local stimuli, which are as appropriate to them as the stimulus of volition is to the muscles of animal life.

But conceding (which is far from being proved) that there is in local diseases either excess or reduction of nervous influence, and granting to these states as much importance as will be demanded in their favour, these states of nervous influence will neither explain the origin nor the phenomena which characterise local disease. If inflammation is produced by external causes, this inflammation is as violent, and will perhaps run through all the stages, or exemplify every ending of this condition, as phlegmonous inflammation, occurring spontaneously, and also exemplifying the course and terminations of this condition. In the first instances in which the succession is palpable, the origin of the disease was not in the brain, or other centre of nerves, but in the part where the injury was inflicted, and which is the seat of the disease. Thus also, supposing the lungs to form, first a tubercle, the disposition must in them be local, even to the very spot where it occurs; seeing that other structures are supplied by trunks and branches of nerves having the same relation with the centre in which tubercle does not take place. The presence of the tubercle excites perhaps suppurative inflammation in the contiguous

structure. Here, if nervous energy is augmented, it is from a local irritation, which constitutes obviously a state of disease which is independent of the modified supply of nervous influence which succeeds to it. Thus also, chronic inflammation occurs in the liver, which, according to the analogy of palpable examples, those of external injuries, originates from a state of disease in this part by which sensibility, or rather perhaps sensation, is augmented; and it terminates in abscess, or ends in resolution, according as its phenomena are directed by that precise state of disease in which the example consists. Thus also, inflammation of the bowels may, as there is reason to suppose, be excited by the presence of scybala, and nephritis by the presence of calculus in the kidney, or vomiting by a stone in an ureter; all exemplifying, that whatever share nervous influence may have in the symptoms, the state of this influence is preceded and governed by a specific condition of local disease.

But admitting the supposition, (which we have seen is untrue) that diseases were always produced by local increase of nervous energy, whether originating in the seat of disease or in a centre of nerves; what variety of nervous energy, whether of muscular power, sensibility, or any other properties ascertained to belong to the nerves, would account for the varieties of disease? We have instances, it may be supposed, conformably with the doctrine, of every degree of local nervous energy in the different grades of inflammation and of fever, to quote the most palpable examples; yet

we find that these degrees all exist without producing irritable ulcers, phagedenic ulcers, the various kinds of cutaneous eruption; without producing tubercle, with perhaps a deposit of phosphate of lime; without forming chalk stones, gall stones, urinary calculi; without ossifying arteries; without producing exostoses, mollities ossium, diabetes, or cancer.

The conclusion from these facts is, that disease may originate in the properties of the textures, which are no further connected with nerves than that this structure forms a part of them; that if nervous influence is augmented in such seat, which in a general way may be questioned (on the ground that the sensibility of a part may be excited when it is not increased,) this increase of nervous influence succeeds to a previous state of disease, and is maintained by, and dependent upon, such condition of local disease; in the same manner as the presence of a preternatural quantity of blood is dependent upon the continuance and condition of disease.

That disease may originate in the nervous structure of an organ, in some instances, is perfectly consonant with our experience, and with *à priori* reasoning. We have seen from the nature or laws of disease, that there is no one structure or set of properties to which the disposition to it can be limited, and consequently none in which the processes of disease may not commence.

We have but few positive examples, except from external injuries of nerves or local irritations (possibly also in tetanus with some reserve,) of the ori-

gin of disease in the branches of this system. In the latter instances, (as from disorder of the muscles, from the pressure of a tumor on nerves, &c.) the disease, so far as this system is concerned, may be secondary, but its phenomena may at the same time be distinct and of chief importance.

Our examples of diseases of this system, originating in their centres, are very numerous. They consist of disease of structure, and disease of function. In the first, disease, although occupying a centre of nerves, is most probably commenced in the properties of organic life, which are independent of nerves. And in the second, disturbed function of the brain seems to have two connexions with phenomena of disease; one which belongs exclusively to its own functions, and one which is relative to disorder or predisposition existing in another seat. The efficiency of the first, in distant seats, consists in excess or privation of the properties traced to this system, which it confers on such seats; which two effects are exemplified in convulsive or spasmodic diseases of the muscular system, from epilepsy, hysteria, perhaps chorea on the one hand, and by paralysis, from apoplexy, on the other. The relative efficacy of disordered function of the brain (the phenomena of which are not expressed by increased or diminished muscular power) seems to be that of communicating preternatural influence to distant seats; which preternatural influence may be related with a predisposition in such seats, and so originate disease in them, or it may establish a predisposition in them of that which we have formerly called its second

or consecutive stage, and thus produce disease in relation with ordinary causes. In this way, disturbance of the brain may sometimes produce dyspepsia, not by communicating or withholding a natural energy, but by the sympathetic influence of a disordered condition, the relation of which is preternatural, and like the sympathetic influence of other seats, engages the operation of preternatural properties.

Our conclusions with respect to the share of the nervous system in disease, taking into the account the evidence of analogies, as well as the direct testimony of facts, are these :

1. That disease, originating either spontaneously or from external causes, may concern principally those properties of a structure which belong to and maintain its organic life.

2. That the function of the nerves of such part may participate in a disease which engages probably, more or less, all the properties of the textures.

3. That the disease may be increased, or modified by the participation in it of the function of the nerves.

4. That the centres of nerves, by nervous connexion, may become disordered by a disease originating independently of the function of the nerves; and that the disorder of a centre of nerves so produced may re-act upon the original disease, or extend the phenomena of disease to other seats.

5. That disease may originate in the branches of nerves, and from a disordered state of proper-

ties may produce the phenomena dependent upon an increase or diminution of their function; or may influence the structures with which they are allied by preternatural properties arising out of their assumed condition; and that in either way, they may direct or modify the processes of disease according to their relation with other properties which may also have digressed from the state of health.

6. That disorder may originate in the brain or other centre of nerves.

7. That this origin of disorder may concern only the function of the organ, and be expressed in remote seats by a modification of the natural influence of such function, which will consist principally in variety of degree.

8. That phenomena of disease of the kind just described may also originate in the brain, or other nervous centre, by secondary disease in the same seat; that is, a process of disease which commences in the organic life of a nervous centre may disturb the functions which belong to animal life, and this effect may be expressed elsewhere in the other systems, exclusively by phenomena dependent upon the animal functions, over which such nervous centre presides.

9. That disorder of the brain, or other nervous centre, originating in either of the two last modes, may extend disorder to distant seats, either by exciting a predisposition in such seats, if the functions of the nervous centre are merely increased or diminished, or by producing disease in such seats, by sympathetic relations of preternatural

properties, dependent upon the condition of disease which has been assumed by the centre of nerves.

It is proper to observe, that this examination of certain doctrines of pathology connected with the nervous system is made under the disadvantages of a doubtful and imperfect physiology, which at present belongs to these organs. We have spoken of sensibility, as if it were clear that a property of this sort is derived from the brain, as from a source. We have no facts which prove this conclusion. We know that voluntary action might originate from an impulse which proceeds from the brain: but the share of the brain in the power even of muscular action is, I presume, not to be defined, although an unsuccessful inquiry, with something like this view, has been attempted. With regard to sensibility, there is no proof that this property is derived from the brain: our experience only is, that certain external agents are so related with properties in the sentient structures, as to affect them, or produce a change, which is communicated to the brain; that the diversities of these changes or affections, resulting from the influence of different related agents, constitute the corresponding shades and varieties of sensation; and that, for the purposes of this intercourse, the integrity, &c. of the nerves is required. There is no proof that the brain first distributes a property which might be called sensibility, to the textures, and that, modified by the agency of an exciting cause, the property is then returned to the source from whence it originated. The property which

is related with the external agent by which sensation is excited may be one of the organic life; or an habitual diffusion of sensibility from nervous centres to the seats of the distribution of nerves may take place. But it is obvious, that so much is at present conjectural with respect to these relations, that on this account alone any doctrines founded upon the presumption of them must be at least visionary, and liable to considerable suspicion.

The nerves are generally considered as the organs of sympathy. I am aware of no facts which prove that secondary or sympathetic disease may proceed from a paralyzed seat; unless certain diseased actions in the lower parts of limbs, which sometimes succeed to the division of their nerves, may be regarded as examples. Whether sympathetic intercourse by nerves is directly between seats, or whether through the centres of nerves, may also be suggested as a question: I believe, however, that sufficient evidence may be derived from the phenomena of disease, to demonstrate that secondary affection occurs by both of these modes. If secondary affection should cease, or not be communicated, after a division of nerves of a primary seat, this would not prove that the course of sympathetic affection was through a nervous centre; because the state of disease may itself be changed by privation of accustomed influence from a nervous centre.

It may be said, in the way of summary, of the nervous system, that the centres of this system may originate an influence, which may be communicated to other parts; that influence, origin-

ating in other parts, may be communicated to distant ones, either directly, by communications of nerves, or through nervous centres. That the influence thus communicated may be modified by the functions of the organs through which it is conveyed.

Whether secondary affection is ever, or in what instances, independent of a course through the nerves, may doubtless be determined by an inquiry on this point. But whatever influence the properties of the nervous system may have in affections of remote seats, this influence is only a part of a causation by which phenomena are produced: the rest is dependent upon properties of the seat in which they are displayed.

With respect to this system, it has been proposed as a question, upon which some ingenuity has been expended, whether the same nerves are the organs of motion and of sensation? none of the proofs which have been quoted on this subject appear to me to be conclusive, since the facts are liable to the explanations of more than one theory. It may be observed, that the same nerve has frequently a different function in the different structures which it supplies; hence, as *common* properties of nerves produce different phenomena in relation with the *peculiar* properties of the seats to which they are distributed, it cannot be inferred that the nerves of sensation and of motion are of different characters, unless it can be shown that they are distributed to precisely the same textures, and consequently that the *related properties* of the seats of their distribution *are the same*.

Nothing tends more effectually to impede the progress of science than the spirit of generalizing, which seems to be actuated by the ambition to form systems. And this vice is the more to be lamented, as it belongs generally to men of superior understandings. The exercise of their faculties is confined by it to a narrow circle; analogies are strained; reflection perverted; observation limited in its extent, and directed constantly to erroneous objects. If any universal laws of nature can be made out, natural phenomena will always be sufficiently within the rules of system, if their coincidence is indicated with such universal laws, with which all particulars should not only be found to agree, but by such laws the investigation of particulars should be assisted. Thus there would be free scope given to observation and inference, and the progress of science would be marked by correct distinctions rather than by forced agreements; and diversities, whether nicely or broadly marked, would be sought after with the care of particular research; and would be discriminated with accuracy; or at least the state of evidence with respect to them would be confessed, and bold assumptions would be precluded.

In a former work*, with the zeal of one who thinks he cannot too much magnify the importance of the subject on which he happens to be engaged, I was a strenuous advocate for the exclusive efficacy of the nervous system, both with respect to life and disease. Phenomena, which could not

* View of the Relations of the Nervous System.

be imputed to the centres of nerves, I assumed to belong to the nerves and their filaments, independently of their centres, and even quoted experiments (which were reasoned upon badly) to prove the truth of this opinion. But I have elsewhere since shown that such opinions, to the extent formerly assumed, are unphilosophical: and that others are better supported. It is superfluous to recapitulate considerations, for the length of which their usefulness in this place would form no apology: and the only one which I shall offer for rejecting opinions which I had once maintained, is, that continued reflection would be idle, and increase of experience altogether useless, if their result were not a progressive improvement.

CHAPTER VII.

RELATIONS OF DISEASE.

Diseases are said to be related, when a disease in one seat produces or modifies a disease in another.

The proof that one disease influences another, is derived from succession; and the evidence of mere succession consists in its analogy to the relation of cause and effect.

Succession is the only proof of the relation of cause and effect: but this relation is not proved in demonstrative instances, to which we should recur for examples, merely by the succession of a consequence to an antecedent; but by a double succession, first of a consequence to an antecedent; and then the cessation of such consequence, on the removal of such antecedent.

Thus, if a lighted candle were introduced into a dark room, the room becoming light is the consequence of the presence of the candle; this is synthetic succession: if the candle were withdrawn, and the room became dark, this would be analytic succession; which last would prove *dependence*. If a glass globe, enclosing a lamp, were introduced into a dark room, the room would become light: the introduction of the globe is the antecedent, the room becoming light is the

consequence; this is synthetic succession: but the lamp remaining, the globe may be removed from the room, and the consequence of its introduction, namely, the room being light, would still continue: this is the mode of analytic succession; and as in the former case, the illumination of the room was known *to depend* upon the presence of the candle, so by it, in the latter case, darkness not succeeding to the removal of the glass globe, which was the visible object to which light in the apartment succeeded, the *independence* of light on the globe is proved. The former case, proving dependence, exemplifies the relation of cause and effect: the latter exemplifies merely succession, without the relation of cause and effect. Indeed, without quitting our first example, it may be observed that, if succession is causation, the candlestick is as much the cause of the light as the candle, for they were both introduced at the same time.

Causation, therefore, is not mere succession, for there might be succession without causation: but succession of the synthetic and analytic kind is the proof of causation, or of the dependence of an effect upon a cause, which confirms the old character of a cause to be, that, without which, the effect would not exist. And this dependence seems to be rather an experience than an inference: for it is witnessed by the senses, that the existence of the effect depends upon the presence of the cause: that the effect exists when the cause is present, and ceases when it is removed: that it *does not*, which is synonymous with *cannot*, exist,

without it; for that which can be, is; and that which is not, cannot be. If it should still be said that our experience in both the synthetic and analytic proofs is merely that of succession, and that the dependence of the effect upon the cause is an inference, I reply, that the succession includes the dependence: in experiencing one, we experience the other; and the term dependence may be used as synonymous with that of succession. But if it be allowed that the effect cannot exist without the cause, whether the dependence is an experience, or an inference, it matters very little: for in point of testimony, an inference, so supported, is not inferior to experience, from which to me it appears impossible to distinguish it.

But if the relation of cause and effect is not proved except by this double process, how comes it that we infer this relation very generally from mere synthetic succession? This, like every other inference, is made from analogy to our experience. We have a general experience that the consequence which succeeds often to an antecedent would cease, if this antecedent were removed, or would not take place without it. From our experience, therefore, that the analytic succession confirms the synthetic in general, when in sensible instances we are able to obtain both proofs, we infer *(cæteris paribus)* a similar connexion in present examples to that which we have experienced in past.

We find that, in causation, the like consequences invariably succeed to the like antecedents; which must be the case in a necessary connexion, or

where, in fact, the existence of the consequence is the existence of the antecedent: or in more familiar language, the existence of the effect is the existence of its causes: and hence we are furnished, in the evidence of synthetic succession, with another point of analogy, upon which the credit of our inference of the relation of cause and effect, founded upon this succession, will depend. In causation, the same consequences invariably succeed to the same antecedents, and invariably cease upon the removal of such antecedents. If, therefore, when the analytic test is precluded, the same consequence invariably succeeded to the same antecedent, we should infer the relation of cause and effect from the resemblance to that experience, in which the analytic test would confirm the character of the cause to the antecedent, which was succeeded by an invariable consequence. And in proportion to the frequency with which the like consequences succeed to the like antecedents, is the analogy complete or incomplete, to the invariable succession which is confirmed to obtain in the relation of cause and effect.

If then a consequence succeeds to an antecedent, as this happens in causation, we infer causation on the ground of analogy just exposed. But as antecedents very frequently precede consequences, which upon repetition are found not to occur, so a single succession of a consequence to a precursor, is sufficient only to indicate a possibility of causation; or, is an inference of the weakest degree of credit. The analogy to the invariable succession which obtains in causation is proved only by the

repeated succession of the same consequences to the same antecedents, and our inference of causation acquires credit, in proportion as our experience proves the analogy in this respect.

The words antecedent and consequence can never stand in the place of the old familiar terms, cause and effect; for although the relation of cause and effect is established by succession, yet these terms have no allusion to the dependence by which the succession takes place. The use of the terms antecedent and consequence, in matters of philosophy, would lend very little aid to analytical research: on the contrary, they would render inquiry loose and erroneous; the business of investigation is to discover dependences, and not mere successions; which latter suggest no reason why the light is given by the candle, rather than by the candlestick.

The great difficulty in analysis is to distinguish between causes, and mere associations, or successions. Causes are those things which are necessary to the existence of the effect, of which dependence we are capable of an experience. Associations are combinations with causes, which have no share in producing the effect, and without which, if they admit of separation, the effect would continue to exist. Successions of the synthetic kind denote an order of events, which may either be related as cause and effect, or between which there is no sort of connexion. If such succession were causation, we should indeed have great reason to feel ourselves interested in the occurrences which are passing at every minute of our lives.

But that mere succession is not causation, is fortunately shown by many examples. Two coaches may start for London, one at six, the other at half-past six: the latter succeeds the former through the several stages, and this order may be repeated every day, and preserved with regularity, for months or years. I presume it will not be said that the coach which goes before is the cause that the other should follow after it: the horses of the latter may perhaps have some share in the business. As soon as the clock has struck twelve, it may be discovered that a house is on fire; the next moment, a fishwoman might cry sprats; the next, a horse in a dust-cart may break his knees; the next, a gentleman may fall down in a fit of apoplexy; the next, a rat may die; the next, a ship may be launched, &c. These are examples of successive phenomena: yet, I presume, no one will assert that the house was on fire because the clock struck twelve; or that a fishwoman cried sprats because the house was on fire; or that a horse in a dust-cart broke his knees because a woman cried sprats; or that the gentleman had apoplexy because the dustman's horse broke his knees; that the rat died because the gentleman had apoplexy; or that the ship was launched because the rat died.

It appears, then, since synthetic succession is not causation, that its testimony amounts only to that of analogy. We have a general experience that, when a consequence succeeds frequently to an antecedent, a dependence or relation of cause and effect would be confirmed by the analytical test; and hence we infer this relation of cause

and effect from the succession which is analogous in one respect to our experience of causation. The rule for appreciating evidence in this case is conformable with the rules by which the value of evidence is estimated on other occasions. The scale by which the value of evidence is estimated may be thus briefly sketched.

Things which are witnessed by the senses are matters of experience, and constitute knowledge. Every thing else which becomes an object of belief, is opinion. Our reliance on the truth of opinions must be in proportion to our experience of the connexion of that which is unseen, with that which is seen. Thus, we infer that night and day will succeed each other next week, because they have done so, without exception, from time immemorial. The inference founded on analogy without exception comes very near to a conviction as strong as that of sensible testimony. If, during ten weeks in the year, the succession of night and day were suspended, and these weeks occurred irregularly, we may then infer that day and night will succeed each other next week; and we should probably be right, because the regular succession occurs four times as often as one suspension. If the suspension happened twenty weeks in a year, the testimony of our inference would be proportionally weakened; if twenty-six weeks in the year, the matter would be so doubtful, that we should scarcely be justified in giving an opinion where the evidence for two contrarieties is equal.

By this rule, all evidence of the inferential kind is to be appreciated. Thus, in medicine we infer

that certain effects are produced by certain causes, from having often witnessed that such causes are succeeded by such effects; and in proportion to the frequency with which we have experienced this succession, the analogy is more or less complete to the invariable succession, which occurs in causation; and consequently our inference is, according to our experience of the succession, well or ill founded.

In related disease, it is concluded that the antecedent is the cause of the consequence, from our experience that such antecedent is often followed by such consequence; or if not precisely by the same consequence, by one, the character of which sufficiently resembles that of the consequence of which we have had experience to admit of an extension of analogy, on the same ground of resemblance.

Our evidence of causation in related disease is, in general, as satisfactory as that upon which we act every hour of our lives, with a confidence which is in general justly reposed. Thus if a person who has been accustomed to hemorrhoidal discharges should have a suspension of them at accustomed periods, and suffer vertigo, pain in the head, &c.; upon an adequate experience, it would be inferred, on the grounds just exposed, from this succession alone, that a relation of cause and effect subsisted between the cessation of hemorrhage from the rectum and the symptoms of vertigo; and this relation would be more perfectly confirmed, if, upon the return of hemorrhoidal discharge, the symptoms of vertigo, &c. ceased. If also a

cutaneous eruption ceased, or was cured, and an internal disease succeeded, as we have abundant instances of succession of this kind, the relation of cause and effect would, on the ground of analogy, be inferred from this succession; and the inference would be strengthened by the additional experience that the internal disease ceased upon the return of the cutaneous eruption.

Experiences of this sort prove a class of diseases which are designated as related; and in assigning any individual instances of successive disease to belong to this class, we do it from the resemblance of such particular instance (of which precise example we have perhaps before had no experience) to the class of which we have had an experience, sufficient to justify the inference of a relation of cause and effect. And in individual examples, the inference of cause and effect is justified in proportion to their analogy to the class of examples with which they are arranged, and of whose character we acknowledge a sufficient testimony. Thus it may be common for disorder of one organ to produce consecutive disease; if a secondary disorder occurred in a seat which we had not before observed it to affect, we should conclude it to be produced by the primary disorder, notwithstanding the difference of seat, if both its succession and character resembled those of secondary disorders, concerning which the relation of cause and effect had been established. But, the succession alone, without this resemblance of character to former experience, would not, in solitary cases, be admitted to prove the relation of cause and effect.

To exemplify: if disorder of the stomach or lungs were observed frequently to succeed to the disappearance of a cutaneous eruption, and to cease on the return of such cutaneous eruption, we should conclude a causation in such instances: if a disorder of the kidneys were to succeed to the disappearance of a cutaneous eruption, as we had before no experience previously of this succession, we should admit that the single point of analogy, consisting of the succession, indicated a causation; but this degree of testimony would be admitted or rejected as the proof of causation, according as the character of the disease of the kidneys exhibited similitude or differences to the other effects which we had experienced to be produced by the disappearance of such cutaneous eruption. The resemblance in this instance would be first in the succession; and in character, it would be observed that the secondary disease in this instance exhibited a disordered function, leading in time perhaps to organic change affecting one seat instead of another; at least the evidence of succession would not be weakened by any difference of character. But, if a person should have a slight attack of erysipelas, on the cessation of which the formation of a steatomatous tumor, or urinary calculus, commenced, we should not, in these cases, infer the relation of cause and effect; because the secondary diseases bore no resemblance, on the contrary, exhibited only diversity of character, to those secondary diseases which we had experienced to be produced by this cause.

There are few instances that exhibit more strikingly the difficulty of discriminating causes than those of related disease. In the first place, there is difficulty in deciding how far the succession proves the relation of cause and effect; and in the second, there is the difficulty of discriminating between causes and associations, the making of which distinction will often be vainly attempted. The stomach may become disordered by the cure of a cutaneous eruption, by drying up an old ulcer, by study, by confinement, by fermented liquors, by anxiety, perhaps by indulging too freely an immoderate affection for turtle, &c. Now the state of dyspepsia, produced by all these causes, may exhibit the same phenomena; and so far the state may be said to be the same. In this multitude of apparent causes, that, without which the effect would not exist, is some property in common, of which our analyses do not instruct us. The same thing happens perpetually to impede the most important research, namely, that after efficient causes, with which the mind can occupy itself: we observe that certain effects succeed to certain causes; and we set down these things in the gross, as the causes of such effects; and they are, according to synthetic succession: but if dependences could be ascertained by the analytic test, we should find many reputed causes of no more consequence to the effect than the candlestick, or the seven inches of candle below the flame, are to giving light to an apartment, or than the iron, of which nails are made, is to keeping together the parts of a box or other mechanism.

Synthetic succession indicates dependence; analytic succession proves it. The evidence of the first is, that a change has occurred from the application of a reputed cause: the evidence of the second is, that the change cannot exist without this cause, or that the former state is resumed on its removal. We are liable to be deceived in both, even where the agents, and the consequent change, are of the sensible kind; both may produce erroneous convictions, as the real causes may be associated and concealed; or they may be altogether independent, and deceive us by a coincidence between the introduction and removal of the alleged cause, and the spontaneous changes, producing the effect, and its cessation, of other agents. Hence, with both these tests, we consider the relation of cause and effect proved only by reiterated experience; and even this is sometimes deceitful; but that we may be deceived by both the tests of causation, does not diminish the credit of the dependence of the effect upon the cause; it only proves that these tests are not applied to the cause, the discovery of which is to be sought by further analysis. Besides which, we are not to reject this testimony, rather than that of experience in other instances, merely because the relations of our senses with forms of existence are so far defective, that the apparent is liable to be mistaken for the real.

To make a full exposition of the topics, and to give a complete explanation of the difficulties which belong to evidence and causation, would be to fill the remainder of this volume with these subjects; which seems superfluous, as I have elsewhere

stated these difficulties, and have treated them elaborately *. On the present occasion I intended to have avoided subtilties altogether; but the brief explanation here given seemed to be required in this chapter for establishing the validity of its title; and if it should have also the effect of stimulating curiosity, this feeling may be fully indulged by a perusal of the book just alluded to. We will merely observe, by way of taking with us, in our further consideration of related disease, the grounds on which this title is conferred, that a relation of cause and effect is assigned to this class of diseases from analogy, with respect both to the synthetic and analytic proofs, to sensible causation. Secondary is observed to succeed to primary disease, which latter may cease on the occurrence of secondary disease, and recur on the cessation of secondary disease. A relation of cause and effect is thus proved, but the mode of this instance of causation is not suggested; instances of primary and secondary disease are inferred to belong to this class, where the analytic proof is wanting, from analogy to the examples which afford this proof, which analogy respects synthetic succession and resemblance of character: and the credit of the inference of related disease is in proportion to the similitude of the present example, with those examples in which the relation is proved and confessed.

In the inference that phenomena belong to the class of related disease, we are liable to be de-

* Indications, &c.

ceived, as in all other inferences: the successive phenomena may be dependent upon each other, or totally unconnected; but here, as on other occasions, we infer that which is unseen from that which is seen, on the ground of a previous experience, of such connexion, provided there are no apparent circumstances by which the analogy is disproved, and the inference confuted, which is founded only on certain points of agreement. In this way, the evidence of synthetic succession alone will be allowed to indicate the relation of cause and effect between phenomena: but it must be observed, that this degree of evidence is to be trusted very suspiciously, where the agents belonging to the seat of the causation are alone sufficient to produce the alleged effects, independently of any influence from a distant seat.

The necessity of recurring perpetually to an abstract discussion on the subjects of testimony and causation is fortunately superseded by general consent and common sense in these matters, which are rarely at variance with the best rules of philosophising; and these rules derive no small credit from their agreement, both in their limited employment, and in their widest extension, with the common sense of mankind, as well as with the most rigid induction.

The class of diseases designated by the term related have been remarked by physicians almost from the earliest records of medicine; and the connexion of diseases, more particularly that between cutaneous and internal ones, has been a popular observation of very remote origin. The vulgar

speak of diseases being cured, by coming out; and of diseases being produced, by their being thrown in, &c. This class of diseases was supposed to reflect considerable proof upon the humoral pathology, by which the relief of an internal, by the occurrence of an external, disease; or the supervention of an internal, upon the cure of an external disease, was imputed, in the one case, to the throwing out, and in the other, to the throwing in, of a morbific matter, or peccant humour. This explanation has been rejected in more modern pathology; at the same time, we continue to have experience of the facts, which are by some either simply confessed, by others explained in a different way, and by others merely arranged.

The relation of disease is sometimes denominated by Dr. Parry, the cure of disease by conversion*; and he has explained the examples agreeably with the theory which he has applied to all other diseases, as a cure of disease in one seat, by a determination of blood to another. It is not here necessary to remark any further upon the term "conversion," than that it is one of some ambiguity. If it is intended by it to express a peculiar mode of causation, I apprehend such peculiar mode is either not defined, or if defined, agreeably with a vague conception of a process indicated by this term, its propriety will want proofs in the present application of it. If the term is meant to designate no peculiar causation, it would be better not to affect such an intention;

* Elements of Pathology, &c.

but to express merely the facts by the terms vicarious, or substituted disease.

With respect to the explanation of these diseases, which is given in conformity with the doctrine of determination of blood, it is necessary only to remark that, as in our examination of this doctrine, it has appeared to be generally erroneous, so there is no reason why an exception to this character should be made in favour of this particular instance.

The curative relation of disease has also been remarked, and particularly insisted upon, by Sir George Gibbes of this city, who has made the developement of this relation the subject of an express treatise. But, from having noticed, agreeably with the observations of physicians, and of the vulgar in all ages, that the occurrence of certain diseases is preventive of some, and curative of others, which preceded them, Dr. Gibbes has the merit of generalising this propensity in nature; and he has concluded, as disease sometimes appears to be curative of disease, that one disease is necessary to the cure of another ("that just so many functions undergo a secondary derangement as are necessary for the cure of the primary one," &c.); and chiefly, that no diseases occur but such as are curative in their effects, or in their tendency.

This doctrine, as a general one, Dr. Gibbes has founded upon the general provision which is observed throughout nature for the preservation of living forms; and he quotes a great variety of illustration to prove that nature has furnished to

all animals the means of living, and of counteracting influences that are inimical to the preservation of their lives; and hence he concludes, as nature has been express in her contrivances to maintain the state of beings whom she has formed, that this tendency in nature is universal; as an universal tendency, that it must belong to man; and that, therefore, his diseases work conformably with this tendency, and are so far from being prejudicial, that they are the means which nature has appointed of prolonging his life. ("Disordered actions are not merely generally, but uniformly, the means which are employed by nature for prolonging our existence," &c.)

In systematising this doctrine, Dr. Gibbes appears to have fallen into two very considerable errors: the first is an inaccuracy of observation with respect to the general tendency of nature; the second is the mistake of opposing assumption to fact, in individual examples.

That all forms of being are endowed, to a certain extent, with the means of preserving those forms, there can be no doubt, simply because forms are preserved for a certain duration, and would not be preserved for such duration, unless nature had endowed them with adequate means. But if the tendency in nature to the preservation of forms be admitted, it must be confessed that this is not the only tendency in nature; that the tendency to *change of form* is no less conspicuous. And as the possession of means of preservation by forms is proved by their preservation; so also it may be concluded, that as things are perpetually

changing their forms, nature has prepared means equally efficient to this end. So far, indeed, from the tendency to preservation being without limit, the only tendency in nature to which no exception has ever been known is that to the destruction of forms; a destiny which has hitherto been fulfilled by productions of every sort, whether those of nature or of art.

This theory, then, of an universal curative tendency in disease, seems to derive but little credit from an appeal to the general plan of nature; and an observation of this plan, which can discover amidst the endless succession of changes which have characterised every period of the world, in every department, only a propensity in things to remain as they are, it need scarcely be told, is indeed very defective, and any conclusions founded upon it must, of course, be erroneous.

If any tendency in nature can be said to be universal, none seems to have a better title to this character than that to change or destruction; which has been universally exemplified in past experience. So strong, indeed, is this tendency, that things are found to contain the means of destructibility within themselves. We see that animals, however favoured by external circumstances, are born, attain maturity, grow old, decay, are decomposed by putrefaction, pass into other forms, and run, perhaps, again through the same series of changes. We see vegetables spring up, flourish, decay, and rot, amidst sunshine, or an alternation of sunshine and showers. If, then, things cannot preserve their forms under the most

favourable external influences, owing to causes which are contained within themselves, how can it be said, that the tendency to perpetuity is universal, or even general, when the opposite tendency is manifested in every possible example?

It is nevertheless true, that things are endowed with a tendency to preserve their forms for a time; and this effect would not be accomplished unless nature had provided the means. But it requires a very choice logic to prove that the means of destruction of forms, which nature has infused with those of preservation, constitute a provision against change. To say the least of it, nature must have mistaken very grossly the propensities of her agents, which is no great compliment to her sagacity.

There is no criterion of *a tendency* but its effects. If things remain as they are, as long as this happens, we may impute to them this tendency; if things change, or their present forms are destroyed, it is presumed that their tendency is to change, or destruction. But, because things are provided with the means of preserving their forms for a time, and are invariably endowed with those which accomplish their destruction, is it to be inferred that the latter are not destructive, because the former tend to preservation?

This is precisely the argument with respect to diseases. Disease is the common medium of death, or destruction; and it proves this tendency by eventually accomplishing this end. But this tendency is not, in all cases of disease, pursued without interruption to its end; or without being coun-

teracted or arrested by the agents of preservation: thus, every form of disease does not tend to death, but most commonly, perhaps, to recovery; and there is a relation between diseases, by which one tending, perhaps, to death, may be substituted by one tending to recovery. But these instances must be regarded as particular examples; and their tendency to cure is inferred upon precisely the same grounds as the tendency of others to kill; that is, by their accomplishing this end; which must, in all cases, be the criterion of a tendency.

The business, then, in considering this class of diseases, is not to generalise, but to make correct distinctions. We find that some diseases are curative of others; but we are not, on this account, to conclude that all diseases are curative, when the opposite effect is their frequent result: nor, because one disease is sometimes cured by another, are we to conclude that additional disease is necessary to cure the secondary one; which series would proceed *ad infinitum*, if the state of disease did not admit the change from disease to health, without additional disease.

In remarking that secondary is sometimes curative of primary disease, and that medicines cure one disease by exciting another, Dr. Gibbes, in common with many who have preceded him, would be right; but not having confined his observations within these bounds, he has the merit of originality in those inferences only, which, according to this view, appear to be made not only without data, but in opposition to facts. Notwithstanding this general condemnation of the doctrine, some good has

arisen from this attempt at systematising, as happens on many other occasions. Men, from attachment to their systems, become zealous observers of every thing which can reflect credit upon them; and they not unfrequently, by showing phenomena in a new or more conspicuous light, serve the cause of science, although they fail to establish their doctrines.

Dr. Gibbes is fully entitled to the benefit of this remark: from observing particularly the curative tendency of diseases, he has indicated the propriety of not resisting secondary diseases in many instances; and of imitating them, when they tend to be curative, in many others. In this latter respect, the doctrine suggests an efficient practice; though it bears a general resemblance to that of Stahl, which was reproached by Cullen, as proposing a mode of curing diseases by expectation: this mode now appears to be revived; and in fashionable phrase it is termed "medecine expectante." It has been remarked by Dr. Gibbes, that some disorders of the stomach are preventive or curative of disorders in other seats; that the reputed cause is, in these instances, frequently the curative effect of disease; that there are many diseases which might be substituted by disorder of the stomach, and so far from allaying disease in this viscus, the only terms of the cure of other connected diseases are to increase it. Thus far Dr. Gibbes' observations appear to a great extent correct and useful; but an enthusiasm in generalising, where the business is discrimination, has led him to erroneous conclusions, even in this example:

and by imputing an efficacy to disease, which belongs not to this state, but depends upon relations of infinite diversity, he has failed to suggest any thing like a principle, by which the phenomena of related disease may be adequately explained; and his observations, so far as they are correct, are remarkable rather for the just importance which is given to them than for their novelty. If Dr. Gibbes had no other merit than that of correcting the erroneous views, and sometimes pernicious practice, of those who will have all disease to originate in the stomach or liver, he would be entitled to considerable praise: but he has gone farther; and has drawn attention to a mode of cure, suggested by Mr. Hunter, that of exciting one disease to cure another, which is likely to be of practical utility, so far as his example and recommendation may extend. The merits of this alleged mode of cure, as a principle, we shall hereafter have occasion to examine.

Inferring the tendency of related disease from phenomena or effects, which we have observed afford the only criterion of a tendency, the diseases of this class may be divided into, 1st, those of substitution; 2d, those of extension. A third class of disease may be added, namely, that which is neither a vicarious one, nor exemplifying mere extension, but one in which the secondary re-acts upon and exacerbates the primary disease: but examples of this sort may be arranged under the title of related extension of disease.

The following cases of the first, or of substi-

tuted disease, will serve as references. A man had severe erysipelatous inflammation of the arm: it was treated by cold saturnine lotions; he had some fever, and became delirious: the delirium occurred in the course of a night, during which the tumefaction of the arm subsided, the temperature of it was reduced rather below that of the other arm, and it became perfectly pale. The inflammation was not renewed in the arm, and the man died of phrenitis in about forty-eight hours. A man had gout, affecting the head, producing delirium, &c.; mustard plasters, in twelve hours, brought on gout in the feet; upon the occurrence of which the head became perfectly free from disorder. These two cases exemplify the conclusions that a secondary disease may happen from a change produced in the seat of primary disease, as in the first example, by the employment of lotions, &c.; or from change in the seat of secondary disease, as in the latter case, by the application of mustard plasters to the feet.

Analogous, so far as concerns the illustration of this case, and at the same time indicative that an irritation of a common kind is, under some circumstances, sufficient to occasion a complete metastasis, is the case of a man who had long suffered from pain in the head. He had severe pneumonia, which ceased suddenly and totally, about the third day, under the occurrence of phrenitis; of which he died in three or four days. On examination it was found that depositions of osseous matter had occurred in the dura mater, in several large patches;

and in these places this investment was firmly adherent to the pia mater: nearly the whole of the falx of the dura mater was converted into bone.

Thus far we state only facts; but it is the business of theoretical, which should serve to conduct experimental, analysis, to trace, if possible, the dependence of these relations more minutely.

If disease happens in a part, which disease involves chiefly its inferred properties of life, the diseased state is a deviation from the healthy state of these properties. As, then, the healthy state has undergone a change, this change must be produced, agreeably with the laws of causation, either by an addition to the properties, or by an abstraction of the properties of the seat.

If this state of disease has no distant relations, it preserves its seat, or it ceases in this seat, or is modified in this seat, either by spontaneous changes, or by external influences. If it has relations by which its influence is extended beyond its seat, its relations are either natural or preternatural; and its influence on other seats is either by communicating, or by abstracting properties from such other seats.

We can assign no natural relation between seats, except that exemplified in the dependence of the function of one part upon another, as muscular power, sensibility, &c., upon the brain. Hence, if disease occurs in such sources of communicated properties, the function of seats to which they are communicated may be impaired in respect to those properties upon which their function is dependent; they may be communicated in excess, their com-

munication may be suspended, or they may be modified. The instances of related disease in general are not those founded on a natural relation; but the state of disease is a preternatural one of properties, which are not, except in certain instances, recognised to have any functional communication with other parts; their relation arises out of a state of disease, and engages in both seats properties equally distinct from those which accomplish the purposes of dependent function. It is not intended to deny the existence of natural relations between organs by this remark, which is meant to apply chiefly to those functions which cannot be exercised except by an influence obtained from distant seats, as those of the animal life, and perhaps some functions also of the organic. In fact, all the organs are a system of subserviency and relation; but this general relation is to the end, and not to the actions by which it is accomplished. Thus, there is a relation between the secerning and absorbent systems; between the skin and the kidneys, &c. &c. This last relation is one which serves as a very general provision against disease in other seats. From the various vicissitudes of temperature, &c. to which the skin is exposed, vicarious determination would happen more frequently to the mucous membranes, if an increased secretion by the kidneys did not occur, in substitution of such determination.

If a disease in one part communicates properties to another, this happens either because the secondary seat has a greater affinity for such properties than the primary; or else, because such

properties receive this direction by powers of impulse in the seat of their origin. But as such powers of impulse at an origin of properties, if one part in particular had no superior affinity for them, would not be distributed to this part rather than to a hundred others, which have the same relation of place, or perhaps greater contiguity; so it is to be inferred that, when properties of disease in one seat are adopted in another, it is by an affinity or peculiar relation which consists in predisposition, in such secondary seat, for the properties engaged in the primary.

The capability in seats to be affected with secondary disease, does not, however, always imply that such secondary seats have digressed from the state of health previous to the occurrence of the primary disease: the affinity in secondary seats for certain properties of disease in primary ones, may be compatible with the healthy state of such secondary seats, as there is always an affinity in health for certain causes of disease, or, in other words, there is a natural predisposition to disease in relation with preternatural causes; but if, from a local primary disease, a local secondary disease is produced, the affinity of the secondary seat for such properties of disease is greater than that of other organs which, although of equal contiguity, may possibly not participate in their influence. Thus, as illustrative of secondary disorder, without previous change in its seat, there is a natural predisposition to disease from poisons, external injuries, &c.; and, as illustrative of an apparent superior affinity for the properties of a primary disease, the

local injury which produces tetanus affects the muscular rather than the membranous system.

Certain primary states of disease are related, directly or mediately, with the whole system; as a compound fracture, for example, in some or other of its stages, in which no part is exempt from a susceptibility of influence from the properties of the local state of disease.

In this relation consists that which in medicine is termed sympathy; and the reason why one part sympathises with another is, that either from natural constitution, or acquired predisposition, the part which sympathises is disposed to adopt, or has an affinity for the properties of a primary disease, and this sympathy displays itself according to the relation either with some or with all the properties of the primary disease.

As in the cases of metastasis, which are the most striking ones of related disease, the affinity exhibited seems to be allied in some measure to a more general agency of this kind, which we observe extensively in all the departments of nature; so, in order to understand the causation in the particular example we are considering, it is necessary first to understand that upon which this agency in general depends. The question to be discussed is, the mode of influence by which things removed affect each other, or sympathise with each other.

It is merely conformable with a law of causation, which will not be disputed, that if one thing produces a change in another, it does so by imparting to it some influence or properties. The

effect or change produced by such influence is dependent upon the relations of the influence with the subject to which it is imparted; or the effect is the form of existence which is made by the union of the forms which are its causes.

If properties are emitted from a substance, or a particular seat, these properties either are or are not so related with the forms of existence within their sphere as to produce effects in conjunction with them. Thus, the properties of odour, emitted by a few grains of musk in a room, are, by their diffusion, applied to tables, chairs, walls, &c.; but with these such properties have no recognised relation, and produce no effects. By this diffusion also, they are applied to the arms, the legs, the eyes, and ears of a person who may be in the room; but with these they have no relation: the olfactory sense of the individual who may be in the room has no other exposure to the properties of odour than the substances with which they produce no effect; but with the olfactory sense these properties of odour are *related*, and produce an effect in the common way of causation.

Thus also a human being who has been robbed, bruised, lacerated, and presenting what is called a shocking spectacle, may be lying by the roadside. The causes which would give to another human being (a traveller, perhaps, who may be passing by) the conviction of distress, are diffused equally among the surrounding trees; the grass is in the sphere of their diffusion; perhaps a horse, water, the clothes, boots, &c. of the traveller. With these the causes of the appearance of a dis-

tressed object have no relation, and consequently produce no effects; but they have relation with the sense of vision, produce by it the perception of human misery, and excite a sympathy. The further effect of this relation is to produce commiseration, which, if unimpeded in its tendency by other causes, will dispose the traveller to approach the object of his pity, to render him assistance, to alleviate his distress.

If the poles of the needle of a compass are made to point east and west, if the needle is for a time retained in this direction, and if on withdrawing the power which so held it, and leaving it free, it moves until its poles point north and south, this action of the needle is produced by the agency of properties. The effect of these properties on the needle is, as in all other instances, according to the relation which subsists between them, or according to the form of existence which is produced by the union of those other forms which are its causes. This instance, in character, precisely resembles those before quoted. That the needle points north and south is imputed to properties of magnetism; the pasteboard of the compass, the wood, the brass, all the surrounding existences, are equally exposed to the properties of magnetism with the needle, but like those properties of odour, or those exciting vision, they produce no effect, unless other properties are within their sphere with which they are related*. The effect is ac-

* This illustration is made conformably with the supposition that there is a power of magnetic attraction in the north pole;

cording to this relation: the traveller would not be drawn towards the object of distress, if he possessed either the apathy or ferocity which sometimes degrades human nature, or if he had reason to fear that by stopping a few moments his benevolence would be impeded, and his own safety endangered: the properties of odour would not produce the sense of smelling if the individual exposed to it had a severe cold; and the magnetic influence would not make the needle point to the north if the steel were amalgamated with a metal not susceptible of such influence.

The conclusion from such facts, with which all others agree, is, that if one thing influences another, it does so by conferring on it properties with which it is related; and the effect of such an influence is according to the relation of the properties engaged, whether to produce the sensation of

but the fact shows only a relation between iron and certain properties of the atmosphere, the result of which relation is that the needle has a direction given to it towards the north. It does not follow that this influence is the same as that of the magnet; but if these powers, from analogy, should be inferred to be identical, this proves only that properties of the atmosphere may be possessed by certain substances, as those which are found to exert the properties of magnetism, from whence the same power may be communicated to other substances, as to iron. But if this illustration of the argument is equivocal, because the theory upon which it rests, and which is generally received, may very reasonably be questioned, this objection will not apply if the magnet is the alleged source of influence, and this source of attraction defined in the illustration, rather than another, which may exemplify only the common instance of a certain effect produced on a substance which is exposed to the action of certain properties.

smell, of vision, to produce commiseration, or a disposition to approach an object, or, by a similar extension of influence related with muscles, an actual approach to such object, or the motion of the needle from a state of rest until its poles point north and south.

Attraction in general acknowledges the same law. If one body draws to it the properties of another, it is because it sends properties to such body, which produce a change in its properties; and this change is a disposition in the properties of one body to pass to another. Thus the properties of sound produced by a drum, those of a modification of light proceeding from a standard, pass from these sources to the objects with which they are related, and produce, as in soldiers, an effect which consists in a disposition to approach the things from whence the causes of that effect emanated.

To inquire upon what law relation depends; why properties of odour should affect the nose rather than the eyes; why the salivary glands should be affected by the sight of food rather than the kidneys; why magnetism should dispose a needle to move; why an acid and an alkali should produce a neutral salt, rather than a fish: to inquire on what law relation depends is, in this place, foreign to our subject. It may be briefly remarked, that relation in general is the effect of existence, or rather belongs to it; and particular relations result from particular forms of existence: to speak of particular relations is only to indicate the form

which is produced by the union of these forms; as two and two make four because two and two are four, or the existence of two and two is four.

Thus we see, that if one thing influences another, it is because it imparts properties to it, with which properties it is related; and the effect, whatever its character might be, is the result of this relation.

But there seems to be a distinction between the influence of one object on a related one, merely by the diffusion of its properties, as those of odour; and the attraction of properties, from one body to another. This difference is only a seeming one: the effect of the properties which are diffused on related ones may be attraction; and if the diffusion is not spontaneous, the separation of properties is produced by the agency of these proceeding from another source. Thus, properties of odour are emitted from a substance from its own dispositions, and influence others within its sphere, as those of the olfactory sense, with which they are related. But if the properties of odour were separated from the substance with which they inhered, or attracted, by the olfactory sense, then the first influence would be from the olfactory sense on the properties of odour, contained in the distant substance. In the one case, the effect is produced by properties which pass from A to B; and in the other, by properties which go from B to A.

But the influence in these cases of re-agency must always have an origin of diffusion in its own seat, independently of distant properties, to which

such influence may be imparted; and the specific effects in distant seats, of such diffusion of influence from a source, must be from relations which are peculiar, and not common to the other substances within its sphere: this peculiar or exclusive relation has been sometimes designated by the word affinity. Thus, the influence which affects A must originate from B, independently of A; or if it does not, then it originates in A, and not in B. Thus the exertion of a power of attraction observes a common law, and the discrimination of order is the only difficulty, in cases which admit the discovery of true or efficient causes.

It may happen that there is a double relation, or a mutual one, between properties, diffused from sources. In the examples of the sensation of smell, produced by properties emitted from musk, this effect is the result of a simple relation; the properties of odour are related with the olfactory sense, and produce, as a consequence, the sensation of smell; but the olfactory sense does not influence the properties of odour, by a diffusion of other properties beyond its seat; that is, it does not attract properties of odour, or deprive surrounding substances of the same share of these properties of odour, as if no such olfactory sense were concerned. Electrical matter, produced in a source, without any influence upon it, would have a diffusion proceeding from its own causes; but it might be attracted by iron, so as to deprive surrounding substances of that electrical matter to which they would be exposed, if the diffusion

were spontaneous. As in the former case the musk is the cause of smell, by emitting properties in the sphere of which is an olfactory sense; so in the latter, the action is from the iron to the electrical matter; and if the diffusion, or course of the electrical matter is influenced or determined by the iron, it must be by a causation proceeding from the iron, and engaging certain properties belonging to it, which properties must influence directly or indirectly, as much electric matter as is attracted to the iron. But the sphere of the diffusion of properties from the iron is uncertain: the influence of the iron on the electric matter may be only in its seat, or place; which influence may be communicated to electrical matter at a greater distance, by continuity, or by a medium through which change or influence is propagated; in which case, the influence proceeding from the iron will have with electricity the relation only of a remote cause: or the diffusion of properties from the iron may be more extensive, and have a direct influence on the whole quantity of electric matter, the laws of the spontaneous diffusion of which it supersedes, and the course of which it determines.

This illustration may to some appear an unhappy one: it will perhaps not be believed: and yet it is as necessary, as that there should be the causes of an effect in any other instance, that iron originates a causation, which may pervade a sphere. But is it more improbable that iron should do this (supposing it to be by the communication of properties to the origin of electric matter) than that a loadstone should diffuse an influence? Since it has been

discovered that magnetism and electricity have some sort of alliance, perhaps the influence of properties of iron on electricity, which may amount to a power of attraction, the properties of which may pervade a sphere, may be the more readily admitted.

It has indeed been concluded that magnetism and electricity are identical, which is just as wise a piece of reasoning, as that electricity is identical with life. The case stands thus: iron attracts electrical matter, and a drum or a standard attracts soldiers: this is a common relation of cause and effect. Magnetism attracts iron; this also is a mere relation of cause and effect, as a fish may be attracted by a bait: because the parts of the relation in either case conjointly produce an effect, they are not therefore to be considered as identical. But electricity will give to other metals the property which distinguishes iron; that is, it will make copper obey the magnet. This proves that electricity and iron have a common property, which is related with magnetism, but which, on this account, is no more magnetism than the properties of odour are the olfactory sense. If surrounding a piece of iron with electrical matter, or if by influencing iron by electricity passing from the south, or if by any true imitation with electricity of magnetic power, a piece of iron, which would not be moved by the common quantum of atmospherical magnetism, may be made to point north and south; this may go some way towards showing the identity of magnetism and electricity.

Returning from this digression, it appears that:

1. If one thing affects, or produces a change in another, it does so by communicating to it an influence, or properties. It will remain to be discriminated, in the several examples, whether the substance which apparently originates an influence is the efficient, or only a remote cause; in the former case, the influence must extend from a seat to the place where its effect is produced: in the latter, it may reach this seat only by intermediate relations, or by a series of connected causation. The former mode has been exemplified in the diffusion of that modification of light proceeding from objects, and in the diffusion of the properties of odour: the latter is exemplified, as a specimen, by the action of a syphon on water, which, so far from originating an influence on the fluid it attracts, produces this effect merely by creating a vacuum in the place which the fluid then occupies: it is also exemplified by the derivation of blood to inflamed seats, and by those innumerable instances, in which a causation, originating in one place, is taken up by related causes, and propagated indefinitely in spheres to which the properties engaged in originating the series of causation do not extend. There is no possibility of discriminating in this, as in other cases, between true and remote causes, unless, as elsewhere explained, the examples admit the application of the analytical test.

2. An influence from one substance on another may be, and is most commonly, communicated independently of the subject which such influence affects, or on which it produces a change.

3. If, in a diffusion of an influence, or of properties from a substance, many things are alike exposed to such influence, but it produce an effect only upon one, it is because one only, among the substances within the sphere of such influence, is related with it.

4. The effect produced by such influence is according to the relation which subsists between it and the substance, or properties with which it is related; and in conjunction with which, the effect is produced.

5. The relation between causes is an effect: and is produced by its causes, in the same manner as the effect itself is produced by its causes.

6. The mode in which causes produce effects is by the union of different forms of existence, which identify, or are, the form which is denominated the effect; as the united existence of three and two is five, while their separate existence is three and two: which parts may again be reduced by analysis, *ad infinitum*, for the reason that the terms of existence are a causation, which is the union of different forms: a conclusion founded on the axiom, *ex nihilo, nihil, &c.*

7. If properties of one substance attract those of another, the order of influence is from the first to the second; and this also, like all others, is a common instance of causation: the effect of such influence from a primary on a secondary seat, is to dispose the properties of the secondary, to pass to the primary seat. This, which seems a peculiar instance of causation, like all others, must belong to one of two modes of causation: the change of

the properties in the affected seat, by the influence of those of the primary one, is produced either by the addition of properties, or by the subtraction of them; and by either of these modes the influence may produce the effect in question, either by a diffusion of properties, extending to the sphere of the primary seat, or by an action upon properties diffused from the primary seat, which action is confined to the secondary seat, and its influence propagated by continuity and relation. In the former case, the influence may produce, in the properties of the substance on which it acts, the effect of a disposition to pass to the origin of such influence, by the addition of properties, or by a combination with related properties, with which those which pass to another seat were before combined, and a separation of this pre-existing connexion. In the latter case, the power of attraction, acting immediately in the place of its origin, may dispose properties to leave a distant seat, and pass to this origin, either by a privation of properties or by an addition of properties, the influence of which is imparted through a distant sphere, either by a propagation through this extent, or by an action upon the successive quantities of influence, diffused from a distant seat; which influence, throughout its sphere, has continuity and relations. The properties thus acting in the secondary have, in determining the course of influence from the primary seat, only the force of remote causes: the efficient or real causes act in the seat where the effects dependent upon them take place.

This scrutiny is for those only who may think it

worth while to understand the relation of cause and effect: which, as a ground-work of inquiry, is the most important of all preliminary considerations. It remains to apply the results of this discussion to the subject of related disease.

Related diseases have been before divided into those of extension, and those which are curative.

1. If a disease in one seat produces a disease in another, it is by a diffusion of the properties with which those of the secondary seat are so related as to constitute the secondary disease. The diffusion of properties or of influence from a primary seat, which, in medical language, may be called a centre, or source of irritation, will produce consecutive disease in as many seats as hold a relation with such influence. If the properties only of one seat hold a relation, there is one secondary disease: if the properties of many, the state of secondary, or consecutive disease, is more general: thus, the presence of a gall-stone in the biliary ducts may produce a secondary diseased action only in organs concerned in vomiting: or it may produce secondary disorder of these organs, and pain in the shoulder; but it may have no relation with the bowels, with the function of respiration, or with the heart; and the bowels may act with regularity in relation to the ingesta retained, the respiration may be tranquil, and the pulse, even during the severity of a paroxysm, may be regular and slow. If we suppose, in this case, that certain nerves determine the diffusion of the influence, which is thus confined to a course, then the order of causation, so far as this effect is concerned, is direct, or

mediate, from the secondary to the primary seat. Thus also, in local injury, as a punctured wound which becomes a source of irritation, the influence imparted from this primary seat may be related only with the heart, accelerating the pulse, &c.; or it may be related with the heart, stomach, brain, &c.; but not with the organs of respiration, with the liver, or other abdominal viscera, whose nerves may be derived from a system, to a certain extent common.

Now in these cases disease is assumed in the secondary seat, either because such seat holds a relation with an influence in the sphere of which are many other parts which are not affected, because there is an absence of relation; or, if this diffusion is not common, but directed, it is from a causation proceeding from the secondary seats: in the latter case, as before remarked, the sphere of influence from a secondary seat is uncertain: it may extend from the secondary to the primary seat; or it may act only in the secondary seat; and the effect, or determination of influence from the primary, may be by the medium of a related continuity: in this case, as before remarked, the influence which appears to originate the causation is not the essential or true, but the remote cause of the phenomena which occur in a distant place.

The dependence then of a mere extension of disease seems to be upon the diffusion of properties from a primary seat, when these properties have the force of causes, which being related with, produce disease in one or more secondary seats. In

this case the primary is independent of the secondary disease; it is maintained by the constitution of its own seat; and its relation with other parts is only to communicate to them an influence which, if they are so related, is capable of producing in them disease. The influence diffused in this case is according to the causation, the processes of which occur in the primary seat; and every variety in the results of this causation may be expressed in the secondary seat, with which these results are related. Hence diseases may be maintained in a primary seat, and diffuse such common properties, as are denoted by the term irritation; or these properties may be more specific or complex, as in the influence diffused from a local cancerous disease, or other of a specific kind; or from a change in the causation of the primary disease, a diffusion of the entire disease might occur; and if such are the relations, the seat of primary disease might return to health, or approximate to this state, while the properties thus diffused express their influence by corresponding change in the secondary disease, which may be to resemble the primary, or to assume its properties, and maintain a similar disease by assimilation.

2. But if a primary disease in a seat is made to cease by a secondary disease, the change in the seat of the primary does not arise out of its own constitution, but is produced by an influence imparted to it by the state of disease in the secondary seat; which influence may extend throughout the sphere between the two seats, or it may be at

the place of the secondary disease, the influence of which is communicated to the primary seat by intermediate relations.

If a secondary affects a primary disease, the result is according to the relation subsisting between the influence originating in the secondary with the properties of the primary seat; and this result will be either a modification of the state of primary disease, or exacerbation of it, or a metastasis, or an examplification of that power of attraction which we have traced in more general or common phenomena.

Thus it appears that primary produces secondary disease, if the influence it imparts is so related with a secondary seat; and the secondary disease is according to this relation. That primary disease may be determined wholly by a causation among the properties of its seat: that it might be maintained, modified, or made to cease by this causation. That secondary may be produced by primary disease, and last as long as the primary disease continues; or secondary disease may be of such a kind as to maintain itself by assimilation, after primary disease has ceased. That the dependence may be exclusively of the secondary upon the primary disease; or secondary may re-act upon primary disease, and exacerbate or modify it, or cure, (or make it cease), by influence direct or mediate; or may dispose the properties of disease in the primary, to pass to the secondary seat, by that power of attraction of which there are in nature numerous examples: and this may be done either by influence communicated from the se-

condary to the primary seat, or by intermediate relations.

It will be no wonder, if a subject on which no one has even ventured an attempt at analysis, should appear complex. Its complexity is the reason why its analysis has never been attempted: and this consideration will perhaps be sufficient to prove that the complexity belongs to the subject, and is not created by the manner in which it is here treated. If this is doubted, let others try an analysis to the same extent; and they will have more than ordinary good fortune if their conclusions are more readily comprehensible.

If there is difficulty in assigning the modes of a complicated causation, there will, I fear, be still greater difficulty in discriminating under which of these modes respective examples are to be arranged; or rather, in particularizing when these modes are exclusive, or mixed. In the gross, the dependence of related disease may be considered under the titles of effects dependent upon primary disease, and effects dependent upon secondary disease: to both of which we may give the following additional illustration.

1. The effects dependent upon primary disease are those which result from a diffusion of properties (or influence); which properties are communicated to a related seat, which is in their sphere, perhaps in common with many other seats, and produce the disease, or the effects dependent upon such relation. This class is exemplified in most of the sympathies: as that of pain in the shoulder from disease of the liver; vomiting from calculus

in an ureter; in disorder of the brain, proceeding from a local disease of a nerve; in some instances of disorder of the stomach, produced by disorder of the brain, in which there is no evidence of reciprocal influence; in disordered functions of certain nerves from disease of their centres, which disease does not deprive such parts of their functional influence, but communicates to them modified or additional properties. In physiology, this disposition of particular seats to be affected by an influence which is common also to other seats is exemplified in volition, as if an energy originated in the brain which is related with and affects only muscles of the little finger; the course of this influence is perhaps through the ulnar nerve, yet no other part of the course or distribution of this nerve is related with the influence of volition proceeding from the brain, which is modified in every instance of its efficacy, except certain muscles which move the little finger. Thus also in a metastasis of gout from the chest or head to the feet, the properties of the disease pervade in their course almost the whole range of organization, yet they find their related properties only in the feet. If, in the instance of muscular action, it should be suspected that the brain has the power of directing an influence to any particular point of the distribution of a common nerve, this supposition is contrary to analogy; it cannot direct its influence to the scalp, or the skin, or even to certain muscular structures; it can originate an energy or influence, which influence can only be conveyed by certain organs; these organs then must produce that

which is not performed by the brain, or that which depends upon them, namely, the direction of a course of influence, as explained in the example of the relation between iron and electricity: and as the action which directs this course does not originate with the muscles of the little finger, so these muscles are affected by a central influence with which they alone are related, which is diffused by related properties in its course, independently of the seat in which its operation is expressed.

In these instances of communicated disease, the primary disease retains its seat, although it communicates certain properties to other seats: and it retains its seat as long as the state of disease is one which is capable of assimilation in this seat. But the state of primary disease may undergo a change by which all its properties are communicated from the same laws, as, in the instances cited, certain ones are communicated, and the disposition to assimilate the disease might cease in the primary seat; hence there would be a metastasis, wherever the properties so separated found a relation, and the primary disease would cease. This is a metastasis from change in the primary seat of disease, and it is exemplified in hernia humoralis, from the employment of injections in gonorrhœa; and perhaps in the phrenitis which succeeded to the treatment of erysipelas in the arm by cold lotions, before mentioned.

2. The effects dependent upon secondary disease, in the relation we are considering, are those which, under an influence which is reciprocal, are produced by the properties of the secondary upon the

primary disease. These instances, in their causation, are similar to those in which primary produces secondary disease: the only difference is that of order. The effect of the influence of secondary upon primary disease is according to the relations of the properties engaged. A secondary may diffuse properties which are so related with a primary disease, as to increase, to modify, or to cure primary disease; or to dispose it to leave its seat, or to cease assimilating the diseased state in its seat. And the sphere of the influence which produces these effects may be either confined to the seat of secondary disease, and exerted upon the primary one by intermediate relations; or the primary disease may participate only in a common diffusion; liable in this respect to the same alternatives of a direct and efficient, or mediate and remote causation, as the examples of secondary, produced by primary, disease.

Specimens of these four effects of secondary, in regard to primary, disease, are furnished in the following examples:

A tubercle in the lungs excites, by its presence, at first, slight inflammation in the contiguous structure: this local irritation, as that of a primary seat, influences, secondarily, the action of the heart, quickens the pulse, augments the velocity of the circulation, and occasions hectic fever: these consequences of the primary disease, which is one of inflammation, simply increase the degree of inflammation, and hasten its progress to ulceration, or abscess. The tumor caused by extravasation of blood,

in aneurism, may re-act upon the artery from whence it escaped, and produce its obliteration; or a local injury, as a gun-shot wound, may, during an exacerbation, from any local cause, as by an exfoliation proceeding to the surface, disorder the stomach; which latter disorder may continue after the local cause has ceased, and maintain a preternatural state of the local injury, either a morbid irritability of the wound, or may so affect it, as to modify or suspend a purulent secretion. Thus, as illustrative of the curative relation, a chronic disorder of the stomach may occasion boils or abscess, and cease under the suppuration which occurs in the secondary disease. A state of artificial disease produced in the abdominal secreting system, may, by intermediate relations, the complexity of which has been here sketched, diminish, and finally subvert, the causes of fever, or of local disease in other seats; and the metastasis of disease from a primary, by the influence of a secondary, disease, is exemplified in that of gout from the head, to the feet or knees, excited by artificial inflammation, perhaps vesication, in the latter situation.

Although the cure of primary succeeds to the occurrence of secondary disease, it is by no means clear, in general, that the secondary cures the primary disease, or exerts upon it any reciprocal influence. On the contrary, it is probable, as we see many instances of the occurrence of curative secondary disease in healthy seats, that the cure of the primary disease has commenced in its own seat, which is the apparent origin of the pheno-

mena, and that the secondary seat becomes diseased by relations arising out of the spontaneous changes in the primary seat.

But if the influence of secondary on primary seats is not clear in the spontaneous examples, the possibility of a mode of cure, by an influence on the primary, originating in the secondary seat, is proved by the instances cited of artificial disease, whose origin in the secondary seat is ascertained by the employment of means which produce disease especially in this seat.

The influence of primary and secondary seats upon each other, by a diffusion or positive communication of properties from one seat to the other, is inferred only in agreement with some sensible examples; but in all, or in some, of these cases, the influence of one seat upon another may be by intermediate relations, and such influence is then not the efficient, but a remote cause, of the phenomena: or its influence is engaged as a part of a series of causation among related properties.

Having traced thus minutely the dependence of disease, chiefly in the hope of inducing those who are inquirers in medicine to make conclusions with less freedom, as well as with the view of furnishing an inferential analysis, which may be preparatory to an experimental one, it is not necessary to recapitulate all the topics of this discussion. We may content ourselves, on the subject of related disease, with the following deductions, which are very little more than an abstract of our experience.

1. Disease in one seat may be related with other parts of various contiguity.

2. The effect of this relation may be to produce in other parts disease, or change of condition, and such disease or change is according to the properties engaged in the relation.

3. The modes by which disease in one part produces change in another, are either by communication of influence to such other seat, in which case it has the force of a true or efficient cause, or by an agency in its own seat on the properties of this seat, which agency leads to change or disease in another seat, by causation among connected properties, or by intermediate relations. The primary state of disease has, in this latter case, with the secondary, the force only of a remote cause.

4. In either of these ways, by direct or mediate causation, the influence of the primary may produce or lead to phenomena in the secondary seat, by addition, or privation of properties.

5. The continuance of disease in the primary seat may be dependent upon its own constitution, or it may be made to cease in this seat, and by intermediate relations with it, may occupy another, from which no influence proceeded: this is metastasis, independently of influence from a secondary seat.

6. Disease in a primary may produce disease in a secondary seat, without a re-agency.

7. If disease in a primary produces disease in a secondary seat, without a re-agency, the se-

condary has no curative or other influence on the primary disease; but the primary disease continues, is modified, or ceases, by the laws of its own constitution; or by agents which prevail in its own seat.

8. A secondary may be produced by a primary disease, and either depend upon the continuance of such primary disease, or it may remain after such primary disease has ceased. These alternatives depend upon its own constitution; and the ultimate dependence of disease in this, as in all other cases, is, whether the condition of disease is a state of life which may be assimilated from arterial blood.

9. A secondary disease may re-act upon a primary one.

10. The effect of such influence of a secondary upon a primary disease, is according to the properties engaged.

11. The modes by which a secondary may influence a primary disease are the same as those by which a primary may produce a secondary disease; it is only an inverted order—that is, it may be either by efficient causation, or communication of properties from one seat to the other, or by remote causation, in which the properties of the secondary may act only in their own seat, and lead to an effect upon the primary disease by intermediate relations or connected causation.

12. By either of these modes, the effect of the influence of the secondary upon the primary disease is according to the relations of properties; and the results of such relations are either,

First, That the influence of the secondary upon the primary disease may be to increase or diminish the degree of its symptoms; or,

Second, That by such influence the state of primary disease may be modified or changed. The effects of this change in the primary disease may be,

1st, A disposition for additional processes of disease;

2d, A disposition to the cessation of disease;

3d, A metastasis, if the fact of its occurrence is allowed to be proved by the succession of the disease in the secondary, to that in the primary seat.

13. That as diseases are related, and liable to these alternatives, so we expect these alternatives, according to our experience of related diseases, or from analogies to those which we have experienced.

14. That as the secondary commonly occurs from processes of change in the primary disease, so its occurrence is frequently the precursor of a cessation of disease, or at least of the previous state of disease, in the primary seat.

15. That if such change does not occur by the properties engaged in the primary seat, it is frequently made to occur by the re-action of those of the secondary seat; and the results of this influence are according to alternatives just expressed.

16. When, from a change occurring in the primary seat, or from influence communicated, direct or mediate from a secondary seat, the primary ceases, while the secondary disease is established; this is an instance of substitution of disease, the

processes of which, in the course of this discussion, have often been described.

A case of secondary substituted disease, occurring from change produced in the primary seat, is the following, which has been before alluded to. A cutaneous disease in a woman, about thirty years of age, consisting both of pustular and vesicular eruptions, extending nearly over the whole body, was, after the fruitless trial of internal remedies, cured suddenly by some ointment: there immediately succeeded tic douloureux of the sciatic nerve; the paroxysms occurred irregularly twice, three, or four times in an hour; they lasted about five minutes, during which she was almost insensible to every thing around her, from the intense agony in the course of this nerve: during the paroxysm her face became flushed, she would roll on the floor, and appeared like a person in convulsions; she then broke out into a profuse sweat, and the paroxysm terminated. This disease of the nerve continued, with some remissions, for many weeks; and it was scarcely, in any degree, mitigated by the numerous remedies which were employed; the only one which appeared at all beneficial was purgatives; bleeding, the narcotics, arsenic, &c., had no perceptible effect. The disease of the skin spontaneously recurred, and the disease of the nerve ceased. The woman remained free from tic douloureux as long as the eruption continued on the skin: this eruption was cured a second time by external applications, and again the disease of the nerve returned, with its former severity. Finally, the disease of the nerve was

partially substituted by a first pregnancy; and both diseases ceased under lactation, and repeated child-bearing.

This case shows, in general coincidence with others of the same kind, that if a disease in one structure is substituted by disease in another, where, from the succession, and other circumstances, there is reason to infer a metastasis, the secondary does not resemble the primary disease, but is a modification of it, corresponding with the differences of function, or properties of the two seats.—A pustular disease of the scalp ceased in a young woman, and was followed by intense pain in the head, which continued many months: if the disease of the brain, or its membranes, had possessed the suppurative tendency of that of the scalp for which it was substituted, apoplexy from the pressure of matter would have supervened. In this case a much more considerable eruption on the scalp was produced by emetic tartar than that of the primary disease; yet the affection of the head was not mitigated by it; which exemplifies the remark before made, that a secondary does not cure a primary disease, *because it is a disease*, which has been by some asserted, but because there is a precise and curative relation among properties which, in the cases of disease substituted for those of a suppurative kind, we frequently imitate in vain by issues, perpetual blisters, setons, ointment with emetic tartar, &c.

But in more natural connexions of functions and seats, it does not always happen that the secondary state ceases on a recurrence of the primary. Of

this fact, together with the conclusion of a substitution produced by primitive change in the secondary seat, there are numerous examples, from which the following case may be selected as one which otherwise manifests a singular connexion of disease. A young lady, about twenty-one years of age, was agitated and oppressed by circumstances of peculiar anxiety; in consequence of which she had frequent attacks of hysteria, and her menstruation became irregular. In connexion with mental disturbance, there came on a copious expectoration of blood, which continued several days, and was followed by total loss of voice. The same causes of anxiety continued; she became exceedingly nervous; had frequent attacks of hysteria, during which she was insensible for perhaps two or three days; the case became chronic: all sorts of remedies were employed, with a direct view to the relief of the chest, as well as to restore the catamenia, but for a very long time without success. This state of disease had continued more than two years, when the case first fell under my care; at which time the symptoms were, total loss of voice, very furred tongue, loss of appetite, reduced flesh, torpid bowels, pulse rarely exceeding eighty, intense pains in the head, frequent attacks of hysteria, buffed blood, pain in the left side, about the situation of the sixth and seventh ribs, with some degree of fulness, menstruation suspended, and an *expectoration of thin bloody fluid, which did not coagulate*, occurring not at regular periods, though frequently at intervals of three weeks, or a month, and continuing two or three days, and

perhaps amounting, on an average, to five or six ounces; it resembled menstrual fluid perfectly in its appearance; it was commonly preceded by hysteria, which accompanied it sometimes for a few hours, sometimes one, two, or three days. It is unnecessary, in this place, to detail all the treatment: it consisted generally of small bleedings from the arm, by leeches, cupping, blisters, purgatives, hip baths, &c. As these means failed, a grain of calomel was given three times a day, chiefly with the design of cleaning the tongue, which effect would succeed, it was anticipated, to a slight mercurial affection; and in the hope that some symptoms might be improved, which appeared to be connected with the state of the digestive organs. At the end of a fortnight the gums became sore, and menstruation returned, which continued for several days so profusely, as to alarm the patient and her friends, after a suspension of nearly three years. But the return of menstruation did not substitute the expectoration, which had been vicarious with it; the chest was but little relieved, and the voice not much improved a twelvemonth after the restoration of the catamenia, the natural periods of which were observed with tolerable regularity. It will perhaps be supposed that organic disease was established in the chest, which perpetuated the symptoms, after the restoration of the catamenia, upon which they would otherwise have ceased. There was, however, no positive evidence of such organic disease: she was sometimes almost free from cough, and could take a deep inspiration

without cough or pain; there was rarely any expectoration, except of the bloody fluid beforementioned, and it was never purulent, which would have been the case most probably, in organic disease, established for so long a period as four years. Against the conclusion, also, of organic disease, it may be further observed, that, under an hysterical paroxysm, the voice, which at other times never exceeded a whisper, was sometimes, although discordant, restored to its natural strength.

In conclusion, it may be remarked, that, if a primary ceases upon the occurrence of a secondary disease, it is not because there is a secondary disease; for one, two, or half a dozen consecutive diseases may succeed to a primary one, which will maintain its seat; but it is from a relation of properties, which relation is liable to those alternatives of tendency before so frequently declared, and among which is the curative; from which, cessation of a primary will succeed to the occurrence of a secondary disease. The order of this curative process may be from the primary to the secondary seat, in which case the primary disease ceases, by change among its own causes: or, it may be from the primary to the secondary seat, with a re-action from the secondary on the primary seat; or the order may be from the secondary to the primary seat; and in these alternatives the causes of either seat may be diffused, and efficient, or confined to their seat, and producing their effect upon a distant one, act by intermediate relations, and have only the efficacy of remote causes. But whatever the order of related change

might be, the dependence of the result is on the relations of the properties of a seat, whether those of its own seat, or those directly or indirectly influential from a distant one.

The instances of related disease are very numerous, in which a secondary substitutes a primary disease by mere consanguinity, or alliance: by which is meant that the character of the primary disease undergoes sudden or progressive change, and the secondary one is the stage in which this change rests, marking an epoch in the processes of disease. There are constitutions which cannot preserve the state of health, but will be erroneous somewhere: and we are instructed by experience that successive states of disease which are allied with each other, but exemplify only a connected causation, may exhibit so great a diversity of phenomena, that even a general resemblance can scarcely be remarked. Dr. Parry has given a most interesting practical illustration of these relations of disease*.

We have hitherto described only the relations of properties in distant seats; but there is a relation of properties engaged in disease, in its own seat, and upon these its changes, period, and ending may depend, whether they are expressed only in such seat, or whether their influence is extended beyond it.

The successive phenomena, whether in the seat of a disease, by relations of its own properties, or by distant connexion, are owing to a causation

* Elements of Pathology and Therapeutics, from p. 368 to 454.

which has been elsewhere designated as one of progressive change *; that is, each new state opens a new relation with the same, or with other surrounding causes. But as this subject has, in the work alluded to, been treated amply, it is necessary here to advert no further to the particulars of the detail. Indeed, the whole subject of related, as well as other disease, has been, in this work, so fully discussed, that very little has remained here to be done, but to extend the analyses formerly indicated, to give a different arrangement to the arguments, or to add to their illustration.

* Indications, &c.

CHAPTER VIII.

GENERAL PRINCIPLES OF PATHOLOGY.

Those doctrines of pathology which might aspire to the appellation of principles, and which prevail at this time, are, I believe, comprised in the preceding chapters. Many opinions are entertained on diseases respectively which can scarcely obtain a notice in this place; for the reason, that our present business is rather with principles, and with classes of phenomena, than with particulars. These individual topics have not, however, been wholly lost sight of, but have been occasionally introduced, rather with the view of illustrating principles than of discussing opinions or facts, which may form subjects of separate consideration.

From the preceding account, which exhibits the error, inconsistency, or inadequacy of the principal doctrines of medicine which have hitherto been proposed, it will appear not at all surprising that men should still be so little agreed upon any thing like a general principle which may serve as a guide to their pathological views. It appears that, without positively defining objections, the common sense of the profession has been generally against the adoption of any one system. At the same time, as all men have felt the necessity of precept of some kind, in a practice which must, even in empirical hands, be governed by opinion, although

the obligation may not be confessed; and as the systems already noticed are the sources of these precepts, so each pathological doctrine has been adopted in part, and the amalgamation forms rather a heterogeneous mass in which truth and error are blended, or else prevail alternately. The fertile source of error in medicine has been one which one would naturally expect to be productive of error: it is that men have occupied themselves in attempting impossibilities. The efforts of systematic writers have very generally been directed to prove that nature is, what they think she ought to be, rather than to demonstrate what she really is. If her operations are not so simple or so regular as we would have them, or if our means of engrafting the phenomena of art upon those of nature are not so certain as could be wished, we shall neither confine her plan, nor render her course more regular, nor obtain over her a more perfect control, by refusing to confess that there are limits to our understanding of her processes, or by hesitating to acknowledge the inadequacy of our means. On the contrary, our views are more likely to be improved, and the condition of our science rendered much more creditable, by justly defining the limits of our information, than by the arrogant presumption that we have nothing more to learn. And yet a presumption of this sort has characterised the doctrines of some systematics to such an extent, that the only anomaly they have left unexplained is, that people should still continue to die, notwithstanding a perfection of knowledge and a

sufficiency of means which should give a physical certainty in the control of this event.

It has been attempted to show (with what success it is for others to determine) that the phenomena of disease are not to be explained by elements, whether moist or dry; by humours, hot or cold; by salts, or by acids, or by alkalies; by solar influence, or by lunar influence; by the spherical or angular shapes of the particles of the animal fluids, or by tubes of large or small dimensions; or by planes or levers. That the explanation of these phenomena is not more complete by a reference to nervous influence; to capillary spasm; to degrees of excitement; to motions and sensations; to superabundance of blood, general or local; to conditions of the stomach, or to states of the liver; to unequal distributions of nervous energy, or to the benefits of disease. Having made so free a condemnation of that which has passed current for knowledge, it behoves us, apparently, to improve upon it if we can; and if not, at least to make the first approach to improvement, which will consist in a becoming confessionof our ignorance.

The first question, in a methodical view, which suggests itself on the subject of disease, is the inquiry in what this state consists?

By disease is meant a deviation from a given state, which is denominated health. This state of health scarcely admits a definition against which some sort of exception may not be taken; and many have therefore attempted it in vain. Health, however, may be said in the gross, to consist in a

due and perfect performance of the function of every organ. The cavils, with respect to this definition, will be, first, to the supposition that it is known in what the function of every organ consists; and second, supposing this function known, as to what constitutes a due or perfect exertion of it, seeing that considerable variety is manifested in this respect in different individuals, who nevertheless appear to enjoy health.

To avoid a multitude of words, let it therefore be said, that the state of health is one which is recognised and determined on by the consent of mankind; that, according to this common consent, a certain state is implied by the term, and that disease is a deviation from this state.

Now in all sensible examples of a deviation from a given state, it is no less deducible from principles of causation, than agreeable with common sense, that the cause of such deviation must consist in something added, or in something subtracted from the subject of it. And, in ordinary instances, or in those which are more level to the senses, we look for this something, and are often capable of particularising it. But in a case where the most important agents admit of being inferred only, and that sometimes obscurely, from their effects, we cannot aspire to an analysis of this sort; we cannot specify what those properties are which make an inflamed seat, or an ulcerated seat, different from a sound one. Instead, therefore, of an inquiry after efficient properties, which, however, is not to be lost sight of in the progress of research, we must content ourselves with classing

facts, arranging phenomena, and deducing from them laws, rather than trouble ourselves with the vain ambition of specifying efficient causes. But in this investigation which respects the laws of phenomena, the nearer we can approach to the specification of causes, the more perfect is our analysis; and any attempts with this view should be encouraged at once, in proportion to the importance of the end, and the difficulty of its attainment. To substitute the definition of causes for the deduction of general rules, is to bring into the sphere of the most perfect vision an object which was before seen dimly at a distance.

A deviation from a given state we observe in other instances is occasioned by something added, or something taken away; and this may be said of the subject in the gross. In this way, a fault in a watch may be expressed; but the object of the artificer is to know, not merely that something is deficient or superabundant in the watch, but to ascertain in what it consists; to discover, probably, that a certain wheel wants a tooth; that there is a displacement of some part of the machinery; that the friction has been somewhere unequal, and a balance probably rendered imperfect; that the main spring is deficient in power, &c. We, in our department of medicine, are precluded an analysis of this satisfactory kind; but we are enabled to particularise sometimes the seats of deviation from our given state, and to propose as alternatives, on which investigation may be employed, the classes of constituents of seats to which, mixed or respectively, such deviation might belong.

A living structure is formed by the union of three sets of properties, which are designated as mechanical, chemical, and vital. The state of health is identified by a certain condition of the properties of each of these departments; it is maintained by their relations with each other; and a deviation in the state of either which is necessary to health may constitute disease.

The history of spontaneous disease is that of a deviation of these properties from a given state; and in order to trace this history, it is necessary to understand the general relations of the properties of these departments with each other.

We observe, that the mechanical fabric, and the chemical constituents of an animal, have no tendency of their own to unite, or to form the organs in which they exist: on the contrary, the disposition of the constituents of these departments is to separation or decomposition. We observe, that without life, on the third class of properties, the two others are not formed into an organized structure; that by it they are first formed; that without it, being thus formed, they manifest their tendency to disunion. We observe further, that there is an immense variety of organized forms; that every one is peculiar, and that this peculiarity is preserved respectively under common means of nutrition, whether they are a direct produce from the earth, (as in vegetables) or whether those organized forms which produce themselves directly from the earth subserve in turn the nutrition of other organized forms. If the chemical and mechanical constitutions of an animal fabric show no

capability of an union without the influence of life; and if, being formed, their decomposition succeeds to the privation of the same influence, and if the instances of organized forms are peculiar, though obtained from a common material; it appears necessary to conclude, that the organization, which would not be but for life, is produced by life; that the organization, which cannot exist without life, is maintained by life; and that the peculiarity of form is not owing to agents which are common, (as those belonging to a common material,) but to those which are peculiar; which must be looked for in the influence upon which the origin and preservation depend respectively of every form of organization.

But, it will be urged, this reasoning is something like attempting to specify the commencement of a circle. It will be observed, there is a parity of argument for the prior efficacy of either set of the properties enumerated. If organization cannot exist or be maintained without life, it will be said, and justly, that we have no experience that the phenomena of life can occur without organization, the testimony from our experience being with regard to these in every respect reciprocal.

If the living state is one constituted by the properties of these three departments, these properties are the causes of the living state, and are all of them as essential to it as causes can be in other instances to their effect. The importance of organization cannot be denied; nay, its indispensability to those functions by which the living state can alone be characterised. Our present object is

not to detract from the importance of any one set of properties, where all are essential; but to distinguish the share which each contributes towards an end, which is their conjoined product, and the result of their relation.

There is no way of determining what share of agency in an effect is to be imputed to any property, or set of properties, but by the analytical inquiry upon what the existence of such effect depends? and as much as cannot be done by one agent must be imputed to another.

To define an example, for the application of this analysis.—An ovum, we will say, is produced (secreted, if the word is preferred) from the vessels of the ovarium, which ovum consists of an aggregation of animal particles endowed with the properties of life. Now, suppose the ovarium, still preserving its organization, to be dead; or suppose its properties of life to be changed, or deficient, as if they were dependent upon a nerve which has been intercepted, would the aggregation of animal particles take place, or would this aggregation be endowed with the properties of life which it may exhibit in another stage? If the ovarium were dead, although it might be injected with blood, and may have all the advantage of temperature; or if its function were modified under which its natural mechanical circumstances may still be preserved, it would not be presuming too much to say, that the material ovum would not in either case be formed, neither would it, if before formed, be endowed with life, unless this principle was conferred at the time of its material organization, which, on some accounts,

is perhaps improbable. The same thing is to be remarked, perhaps more satisfactorily, with respect to the function of the testes, which is familiar as an ordinary example of secretion. If, then, the ovum, or the secretion in the other case, would not be formed by the organization, its formation must be imputed to the life of the seat, which is the only other recognised agent besides the organization.

Now, to exhibit the argument fairly, that is, both sides of it, it must be asked, would the ovum or the secretion be formed by the life of the part, without the organization? It must be replied, no. —The conclusion is, that the formation of the ovum or of the secretion depends on both.

To continue the analysis: it is to be asked, what the organization of a seat performs independently of its life, or, as results of a mechanical construction? All besides has no dependence upon it. The organization, it must be answered, supplies blood, and such a vascular arrangement as to admit the formation of an ovum, or of the secretion, by the influence of life. Thus far, the organization is indispensable to the end. What remains of the process is, that which organization is incapable of performing; and that which remains is done by the only other recognised agent, namely, life, and consists in the aggregation of particles, which would not be aggregated without it, and in the endowment of these particles with life, which they could not otherwise obtain. Because both organization and life are necessary to the living state, it does not follow that each has not its share in this end, or that the share of each may not be defined.

To recur to a former example: the walls of a room are necessary to constitute a lighted apartment; but if darkness supervened upon the removal of a candle, it may be presumed, that although the walls formed the apartment, the candle gave the light: this independent efficacy of the parts respectively which concur to an end is exemplified palpably in innumerable examples, and really by effects of every kind.

It appears then, if this analysis is true, that the dependence of the phenomena of life upon organization is that of a fitness of mechanism for the operation of another influence; that all the phenomena of life are dependent upon this other influence, which cannot be imputed to mechanism; which latter is nevertheless necessary to these phenomena, because without the organization or mechanism, the power which accomplishes the other part of the phenomena is nugatory. Hence, it is true, that the chemicals of a structure would not be united if they were not supplied; but that their being united, when they otherwise tend to separation, is owing to the vital properties, to whose influence they are subjected. It is true, that organic particles would not be laid down in structures for the purpose of growth or renovation, if such particles were not duly supplied by an organization adapted to this purpose, but that, if the influence of life did not settle their place and incorporate them in the textures, they may be supplied to all eternity, and remain, if they were not decomposed, as organic particles in their fluid medium. It is true, that if the earth did not

supply constituents, the productions whose being she maintains would fail, but that no peculiar productions of the animate kind would be originated from her, but for the influence of the peculiar properties with which vegetable forms are endowed, and which give to each form a respective relation with the common materials of the earth. And it appears that this co-operation on the part of organization is all that is necessary to the phenomena of life. The organization supplies (and this not without aid from the other department,) a material, and is passive; life acts upon this material according to its precise form, and in turn exerts its influence on the organization with which it is allied. But a very precise organization does not, at all times, appear necessary for the accomplishment of these phenomena, or of that part of phenomena whose dependence on life may be traced. We see organs greatly modified, still performing their functions; and in the nutrition of the chick, the material seems to be supplied without the aid of organization for this purpose. To what extent this government of life is exercised over its material alliances is perhaps uncertain; or whether it may not have happened that life has, at some period, formed organization from a material, without the aid of previous organization. This consideration has elsewhere* been pursued at some length. It may here be observed briefly, that the independence of life on organization is proved by the circumstance that it exists in unorganized

* Indications, &c.

bodies, from which sources it is obtained by living forms.

It appears then, that the efficacy of organization is to supply a material which, previously to the peculiar separations which are made from it by the agency of life, is so far a common one that all parts are supplied with it; and this material is blood. Now, if from a common material a peculiar product is formed in any seat by related properties, (which product would not be formed without such properties,) it follows that the product is such as it is made by, or is according to the peculiarity of the related properties of such seat. Thus, if blood is conveyed to the kidneys, and urine is produced from it, this product, which would not take place but for the life of this seat, is, according to the precise agency of the form of life which is exerted in this seat. If the same common material is conveyed to the liver, and bile is formed from it, as this formation is owing to the life of this organ, so the life in this seat being different from that in the kidneys produces bile, and not urine. If blood, the common material, is also conveyed to the ovarium, or the testes, and a peculiar production is also formed from it, this production takes its nature or character, not from the efficacy of a material which is common to other parts, but from the agency of the related properties of life which are peculiar; and every shade, modification, or peculiarity in such production must be owing to a corresponding difference or modification of the related peculiar properties which act on a common material. And this dependence would be equal, whe-

ther the product were formed from a single system of vessels in the organ, which produces it, or whether a dozen sets of vessels could be enumerated, which would only be so many subordinate systems preparatory to the end. Analysis will still lead to a common material; and the dependence of peculiar products will still be traced to the peculiar properties which are related with this common material. All, therefore, which organization cannot do, I think it may be justly concluded is done by life; and this is every thing, except the supply of the material, and to this also it concurs. It compels the union of the particles which would not otherwise unite; its relation is precise with these particles, and it determines their nature, or it separates them, by a relation with them from a common material, rather than others with which it is not related. If these particles have an arrangement, they would have no arrangement without life; on the contrary, they tend to disunion, and would run into decomposition. Their arrangement then, if they have any, is according to the influence of life. If they are endowed with properties which are peculiar, or corresponding with those of the subject in whom they are produced, they do not derive them from a common material alone, by the efficacy of which they could not be separated, but also from those related ones of life which are peculiar. And this must happen, whatever the nature of those properties may be; whether their disposition is to maintain their present state, or to admit chemical changes; or, as in the form of the ovum or seminal radicle, to run through

other stages of life, to the phenomena of which they are predisponent, in conjunction with the external influences and internal changes to which they are subsequently exposed and liable.

It remains to connect this physiological digression with the subject of disease: the first question with respect to the history of which we have said to be concerning its spontaneous origin.

Disease is frequently attributed to external causes, or to new and sometimes to habitual external influences. This reference seldom explains disease; but our business is not with disease produced by arsenic, or any other preternatural agent. To trace the laws of disease, it requires that we should consider its spontaneous origin, with which disease from the external causes also, to which it is often imputed, will be found to be very closely connected.

We will suppose a disease to occur at the age of forty (or any other age), under the continuance of the same habits as have obtained, and were compatible with health, for the preceding twenty years, and which, so far as health is concerned, may be unexceptionable. Say the disease is chronic inflammation of the kidneys, or of the liver, tending to abscess. As the disease, we suppose, was not produced by any external preternatural influence, how did it arise? from a disposition, it must be said, to chronic inflammation in this seat. And how came this disposition to exist in this seat, at the time it was displayed, rather than a fortnight before? from some change, either in the constitution of this seat, or of a related one. And

how came this change to happen? also from a disposition to it. And this disposition? produced by previous change. In this way, all spontaneous internal phenomena may be traced to the ovum. From which beginning, phenomena are developed, according to the properties possessed by the ovum, and their relation with the material of nutrition, and with external influences. Every phenomenon which is not produced at once by a preternatural agent, or which did not take place under previous exposure to such agent, is owing to a predisposition, with which the ovum was originally endowed, in relation with external influence, and that of nutrition.

But the spontaneous changes, whether of one seat, or prevailing in many seats, are not recognised in the stages of predisposition, because they have no relation with the senses: they must proceed to that additional stage of change, which we call disease; and the condition is then recognised by signs, or symptoms.

The general origin of disease may therefore be said to be in a predisposition, which may proceed from a causation, the series of which begins with the constitution of the parental radicle, and proceeds by relations with the agents of subsequent life, to which this constitution is exposed. Any peculiarity therefore, which is displayed in subsequent life, under circumstances that are common, is to be imputed to a causation which commenced from the original constitution of the subject. It may be traced still further; as to parents, perhaps for many generations; but this is superfluous.

Now it has been proved, according to our previous analysis, that the mechanical and chemical constitution of the ovum is according to the properties of life which affect it, by which it is produced from a common material; and that its vital properties are obtained from the same source. If, then, the peculiarity which may originate a predisposition to any particular phenomena should be said to depend upon the material constitution of the ovum, this is confessing the dependence upon the spiritual or vital influence by which the ovum itself was formed: a similar dependence is also confessed, if the peculiarity which originates changes of predisposition, terminating in disease, or any thing else, is said to belong to the department of vital properties, with which the ovum is endowed. The dependence of such progressive changes or predispositions upon life, in the former case, is mediate; in the latter, it is direct: the material fabric or organization is in the former case, at this remote period, as in the stages of subsequent life, only a secondary cause, or a re-agent.

The progression which was begun at the first era of individual existence, its order and relations, are afterwards maintained. Phenomena originate in vital or spiritual agency; a series of changes may occur in this department, exclusively: (at least we have reason to think so, from the history of predisposition) or the constitutions allied with the vital properties may participate in them, and add to the complexity of the causation, in the way of secondary causes, or re-agents; or may produce other effects in their own department, or may be

combined in agency with the properties belonging to that of life. In short, spiritual influence is primitive; the stages of the organization, with which it is allied, are secondary: but a relation subsisting between them, they may mutually interest each other; produce phenomena in conjunction, or perhaps separately; and their reagencies may be indefinitely interchanged and mixed.

Without bestowing any further considerations (which are elsewhere sufficiently ample*) upon the intricacies of predisposition, the nature of which, for purposes of reference, it was here necessary to indicate, we will suppose the state of disease to be established: and we will apply our analysis, (1.) to its condition; (2.) to the relation of agents which constitute its condition; (3.) to its relations as a cause; (4.) to the laws of its continuance, and varieties; (5.) to those of recovery, or of death.

1. In our attempts at a definition of disease, we could approach no nearer to a precise account of it, than that it consisted of a modification of health; which modification was displayed by certain signs or symptoms. We have ascertained that a certain condition, both of the life of structures, and of its alliances in the mechanical, chemical, and hydraulic departments, is necessary to health; and, consequently, a deviation from this condition, in the agents of either of these departments, may constitute disease. But, as the properties of these

* Indications, &c.

departments are intimately allied, and related with each other, so a distinct or exclusive modification of either is rarely displayed by symptoms. The properties of the mechanical, chemical, and hydraulic departments, may be spoken of separately, or they may sometimes be referred to, under the inclusive terms structure, or organization.

The life of a seat, in the state of disease, is changed; in conformity with general principles, we may infer that this change is produced by addition or subtraction of properties, but we cannot say to what properties the change is to be imputed, or in what the change consists. It has been attempted to limit the varieties of the condition of life to those of degree, but it most commonly happens in disease, that effects are produced, or symptoms exhibited, which cannot be accounted for by any variety in the degree or force of properties of life; on the contrary, such symptoms are generally to be explained only by supposing the operation of new or different properties, or of such as are preternatural, in the seat of disease, rather than by any variety in the degree of those which are natural. As for example; what degree of a natural property constitutes pain? (an instance the least favourable.) We can say of pain that it is a modified sensibility; but to say that it is an increased sensibility, would be just as reasonable as to say that the infinite perceptions of a sense, as that of vision, are only degrees of the faculty of perception.

But if pain does not exemplify the degree of a natural property, (by which it is not intended to

deny that there are different degrees of pain), to the degree of what property shall we refer the formation of pus; the aggregation of adventitious matter in a swelled gland; the deposition of phosphate of lime in the membranous structures; the formation of biliary concretions; the production of urinary calculi; the various phenomena of ulcers; or even the states of blood vessels which dispose them to rupture? And if these circumstances should, by dint of assumption, still be tenaciously referred to degree, to what are we to impute the diversities, cause, and duration, of diseases, the degrees of which, so far as they can be estimated by effects dependent on degree, are at some period the same? and if their phenomena afterwards diverge, it may be guessed that there is a causation going on in the constitution of disease, which determines degree itself, rather than that the disease is one only of degree, or that its phenomena are dependent upon degree.

As disease exhibits symptoms, or effects, which can be produced only by a change in the nature or properties of the life of its seat, so these effects must be imputed to such change, which is inferred; but the nature of which does not admit of being specified, the properties having hitherto eluded all our approaches to an intimate acquaintance. As certain effects are also exhibited by disease, which are dependent upon degree, as power of digestion, muscular power, perhaps tonicity in the arteries, &c.; so the degree of natural properties, exemplified in disease, must be allowed this importance: at the same time it is to be re-

membered that, although certain effects are dependent upon degree, degree itself is dependent upon previous changes of the constitution of life, which changes are characterised by other symptoms, or else, in other respects, rest in the state of predisposition, although the single sign of disease, which may consist in increase or loss of power, is sufficient to show that predisposition has gone into that stage of change which constitutes disease.

Thus it appears, that disease of the constitution of life, or, as I have elsewhere called it, of the organic spirit, consists of a change, the nature of which cannot be defined, and consequently we are permitted to reason upon it, with a view of deducing its laws only, from our experience of its effects, or signs.

The condition of disease which respects the chemical alliances of life may consist either of a modification of the nature of chemical constitutions, which subserve the perfection of a living structure, or in the formation of new or preternatural products. As seats of disease can never be subjected to chemical analysis, and as the means of analysis in this department, which have relation with living bodies, are totally inadequate to any useful application of them, it seems unnecessary to say any more on this subject, than that, as the state of health is maintained by the concurrence of a precise chemical constitution, so one part of disease may be a deviation from this precise state. The deviation may be said to consist either of a modification, or change, of the natural chemical

constitution, or in the formation of preternatural products. With respect to the former, an investigation can scarcely be proposed: with respect to the latter, analysis has succeeded to a certain extent; but as this analysis has not been one of disease, but of the products of disease, so pathology has been but little elucidated by these attempts; and the therapeutical views which have arisen out of chemical investigation of the products of disease, have been founded upon the false principle of applying a treatment to the effect, instead of the cause of such disease; and hence such treatments have never succeeded, unless the means employed have had a relation with the cause, or general condition, of disease, which was not contemplated in their exhibition.

The condition of disease with respect to the textures may be said to consist in a modification of the natural organization which subserves the purposes of life and health; or in the destruction of the natural fabric; or in the formation of preternatural growths. As the textures are governed by the life which is allied with them, so their changes are secondary, and in the phenomena which they produce, in relation with life, they have only the force of re-agents.

The condition of disease, in the hydraulic department, is liable to varieties only of quantity and place: these varieties may respect blood, or the secerned fluids. There is excess of fluids in those which are called the seats of determination, and privation of them probably, in certain cases of paralysis, and also in seats which suffer diminu-

tion of blood, from their relation with a seat of preternatural derivation of this fluid; as perhaps in the uterus, in consequence of the derivation of blood to the head or chest, accompanying hysteria, or hæmoptysis; in the feet, in almost every affection of the head, &c. The varieties of place also respect blood, or the secerned fluids: they are exemplified in the extravasation of blood which is a part of disease, and by the secretions into the cavities of the peritoneum, tunica vaginalis testis, pleura, pericardium, ventricles of the brain, cellular membrane; in the situation of pus, whether resting on bones, or pervading textures, otherwise united, forming sinuses, &c. The unmixed efficacy of varieties, either in the chemical, mechanical, or hydraulic departments, is not to produce the signs of disease, or symptoms, but states which continue after death, and are among the phenomena of bodies which never possessed life.

2. It very rarely happens that change in the condition of the life of seats is not accompanied by change in its material alliance, which latter helps to constitute the condition of disease. Among the most frequent deviations from the natural mechanical state, is that of increased vascular fulness. Indeed, in the chapter on determination of blood, we were able to mention very few cases which did not exemplify this general accompaniment.

In every state of disease, even if there should be no signs of change in the material department, the concurrence of the organization is necessary to its existence, as it is to that of life itself. Without deviation, then, from a healthy constitution,

the mechanical and hydraulic departments subserve the establishment and maintenance of disease: but if signs of disease are also exhibited in these departments, one of two results may be remarked; either, 1st, the phenomena of disease are merely multiplied by the influence of a changed condition of the properties of life, on their material alliances; or, 2d, the phenomena of disease are multiplied, or its condition altered, by the re-action of the effect produced on the organization by the influence of a primary change in the condition of life. We shall have occasion to notice that this re-action is almost universal in disease, although it may not be attended by disorganization.

Considering then disease as an effect, the causes which produce it, or which are necessary to it, are those belonging to all the departments which are necessary to life and health, of which disease is only a modification: and the concurrence of all these causes is necessary in the twofold relation, 1st, as the phenomena of disease are the products of the living state, in which the concurrence of all the parts is necessary to maintain the existence, whether natural, or changed, of one: and, 2d, as the changes of condition in any one department may be related with the others, and produce in them corresponding changes, which may multiply only the signs of disease, or re-act upon the properties of that department in which it originated.

3. The causation proceeding from disease may involve the reciprocal influences of all those properties which we have said to belong to the three

departments of vital, chemical, and mechanical constitution, as well as all those relations of disease, the history of which has been sketched in the preceding chapters. In considering the state of disease as a cause of phenomena, our first business is with the primary change in the department of life.

A deviation in the healthy condition of life has been said to be the result of a progressive change, proceeding among properties of life, which progression consists of a series of changes, each opening a new relation with connected properties, and constituting a predisposition, in which state the changes are not recognised by the senses, or are not displayed by symptoms. The same progressive causation being continued, predisposition goes into the stage of disease, either under habitual influences or with the aid of an exciting cause.

The life of a seat, as may be demonstrated, consists of an infinity of properties: a certain constitution of which forms a state of health in this department. If this constitution is changed, the relations of the properties with each other are also changed: and these changes, in the relations of properties of life, are manifested in disease by corresponding changes in the organization, with which they are also related. Hence, the first relation of disease, as a cause, is to be considered with reference to the properties of the department in which it originates.

As the properties of life in a seat are united and related with each other, so any deviation from the natural state of these properties must produce ef-

fects, or changes, agreeably with the relations subsisting between these properties, and the preternatural state which they have assumed. According to these relations would be the results of disease, if the processes of it were confined to this department. But, in general, the relations between states of spiritual properties are modified, or interrupted, by the re-agency of the material alliances.

If a disorder were purely functional, its course would depend on the relations only between vital properties. But it may be doubted, whether in so intimate a connexion of agents, a condition is ever so perfectly exclusive: the common effect of primary, or spiritual change, is to produce a corresponding change in the fulness of the vascular system of its seat: the influence of the primary change may produce only effects displayed by sensible appearances of this system, or it may produce disorganization, or preternatural growths or depositions.

The secondary disease then acquires the character of a cause; and in turn may modify the primary spiritual changes, or may produce specific phenomena, in the spiritual department, according to the nature of the secondary disease, and its relation as a cause with the department of life. A similar reciprocal dependence obtains between the vital and chemical, as that just remarked between the vital and mechanical states.

The relation of a cause, displayed by mechanical re-agencies in spontaneous disease, demonstrates a possibility of an origin of disease in the organization. In this case, the relation of life with the

cause of such disease is mediate, and the organization serves to extend the relations by which changes in the department of life may be produced.

But such an origin of disease in the organization must commonly, or perhaps always, arise from a mechanical relation of the structures with externals of the same class, and the instances do not therefore come under the denomination of spontaneous disease.

It is probable that a mechanical agent, whether external or belonging to the organization, never produces spiritual change by a direct relation. We can scarcely conceive, that the nature of a principle, which is neither itself, in the gross, an object of the senses, nor has sensible properties in its constitution, should suffer a change in its nature by the configuration or pressure of mechanical agents. But in the living fabric, there is a relation between the properties of the life of minute spheres, and these properties are allied with the organization: any change, therefore, in the organization, may produce phenomena in the life of seats, by occasioning preternatural spheres among related properties.

We have seen, that the state of spontaneous primary disease is related as a cause in its own seat: first, with the properties of the department of life; second, with those of organization; that the state of disease in the organization is related also as a cause with the entire constitution of its own seat.

Disease, as a cause, or in relation with consecutive disease, has a similar importance in regard to

distant seats. Secondary disease in a distant seat may be produced: first, by direct relation between properties of life in the two seats; second, by the re-action of secondary or structural disease in a primary seat, the influence of which is communicated to a distant related seat.

The relation of disease as a cause of phenomena observes the same order in a secondary, as that which has been assigned to it in a primary seat. The spiritual change in the secondary seat is related with properties of its own class, and with those of the organization with which they are allied. The change in the organization might in turn obtain the character of a cause, and modify the state of disease or the phenomena of life in the secondary seat. When the relation of states of different seats is between properties of life, the agency is direct; when properties of a primary influence those of a secondary seat, owing to the agency of organization in the primary seat, the relation, as a cause of the change of organization in a primary with the condition of a secondary seat, is then indirect or mediate. All these relations have been before exemplified, and a repetition of their instances is not only unnecessary, but in a mere summary or abstract would be misplaced.

4. The continuance of disease, which consists of spiritual change, depends, as has been said, first, upon the causes which produce this change. But an important or characteristic difference may be remarked of these causes.

We have arrived at the conclusions in our phy-

siology that there is no fixed or permanent sum of life, but that this principle is perpetually dying or changing its form; that each quantum renews itself by the assimilation of its identity from blood, or a material which contains its elements, and then becomes informal; each sum renews itself, passes away, or becomes informal, and is succeeded by a new quantum of a similar principle, produced by itself from a material by the separation, and the union of its elements. It has been elsewhere* briefly said of this process of assimilation, "life is, renews itself, and dies."

Now, if the natural state of life has undergone the change in which disease consists, this change is only a modification of a principle which is perpetuated by successive renovations. As the properties which constitute the change are united with, or make a part of this principle, the conditions of the continuance of disease are either: first, that the state of life under such change should be one capable of assimilation, by which the properties of disease, in common with those others which belong to the natural state of life, are produced or renewed from blood; or, second, that the properties of disease should be constantly supplied from a source which is independent of a process of assimilation.

Thus, to exemplify the first; in inflammation, or in cancer, or in any disease the phenomena of which may be traced to primary disease, or change in the spiritual department, the diseased condition is one of life, which is capable of assimilation, and

* Indications, &c.

in this way it is maintained. Examples of the second, or of those affections of life which are independent of assimilation, are furnished by the effects of preternatural agents, and by secondary dependent disease, or the re-action of a change of organization or preternatural product on the department of life, which is that to which primary spontaneous change belongs. Thus drinking inordinately of wine or other spirits may produce fever, and a pulse perhaps of a hundred and twenty. So also will exposure to cold; or hectic fever, occurring, perhaps, without such exposure. Life, or the cause of the phenomena of fever, (of which state the action of the heart may be taken as a representative,) in the former case is affected by a preternatural agent; and this state continues only so long as the cause acts, or until it is expended. In the latter case, the affection of life is not owing to a preternatural agent, the sum of which is fixed, and the effects of which cease when this sum is expended; but the condition of life which gives rise to the phenomena of fever is one which is capable of assimilation: and in this way it is maintained independently of any other source. It may happen, that the preternatural agent, whether alcohol or any other stimulus, is so related with certain predispositions, that the state of fever once excited by such agent, is afterwards maintained. The difference is, that when this state is not assumed, the phenomena of fever cease when the properties which excite it are consumed; when this fever is maintained, after the operation of the stimulus has ceased, it is because, with the aid of a predisposition, it has produced a state of the

pr[illegible]le capable of the phenomena of fever, which [illegible]petuated by assimilation.

Thus also, secondary disease, consisting of modified structure, or preternatural growths, or deposits, may maintain a state of disease in the department of life, which does not depend upon assimilation; that is, the diseased condition in this department having ceased, the assimilation of life may be natural, but displays preternatural phenomena, owing to the re-action of secondary disease. The state of the principle under these circumstances may be one only of affection, which would cease as soon as the cause is removed; as in calculus in the bladder, gall-stone, the stricture of hernia, &c.: or the relation of secondary disease with the department of life may be to produce a diseased state of this principle, which is capable of being maintained by assimilation; as when fever continues after a cause of irritation is removed, as in diseases of joints, compound fractures, gun-shot wounds, &c.

According to this view, the continuance of a changed or diseased state in the department of life may be said to be upon assimilation and affection. The causes of disease in the first are renovated, as part of the state of life; in the second, they are supplied to life by the mechanical influence of changed structures or preternatural products.

The distinctions on this subject, for the purposes of a full exposition, admit of being still more numerous and refined. Many of these instances of disease, which appear to be maintained by change of organization, are, in fact, dependent

upon an assimilating state of life. Thus, there is a change of structure in schirrhous liver, or in tubercle of the lungs, or in a swelled gland; and these changes of structure may re-act upon the principle, and modify the phenomena of life. But if the principle in these seats did not assimilate the condition of it, which maintains the change of organization, the phenomena of life produced by the re-action would not take place; since the structure would return to its natural state, if the properties of life which maintained its former, or diseased state, were incapable of being renewed by assimilation.

Although, then, the effects of primary disease, expressed in the structures, may be capable of a re-action, yet the change of structure itself is dependent upon assimilation in the department of life. In tracing the dependence of disease upon this assimilation, it is necessary that we should distinguish: first, the effects of the re-action of the altered structures; and, second, between preternatural products, or changes of structures which do, or do not, depend upon the assimilation of the principle in their seats.

We have no modes of discriminating the effects which are produced by affection or re-action but the analytical test before spoken of, and by analogy. We may judge that disease is maintained by a material cause, or one independent of assimilation, when effects cease upon the removal of such cause, and by means which do not alter previously the condition of life, and consequently its assimilation; as when disease ceases upon the escape of a

gall-stone from the biliary duct, whether by passing into the intestines, or by falling back into the gall-bladder; or as when disease ceases by the removal of a stone from the bladder, or by the removal of a stricture upon a portion of intestine, or of a distant local disease which occasions epilepsy, &c. This distinction is also exemplified in general by the exhibition of medicines, the effects of which cease when their properties are expended. Where this analytical test is precluded, we also judge that phenomena depend upon the re-action of material causes, or upon affection, by analogy of the mechanical circumstances to those which produce the same effects. Thus, difficulty of respiration is produced by ascites, impeding the descent of the diaphragm; which effect would cease upon the removal of a mechanical cause. Thus also, apoplexy is produced by effusion into the ventricles of the brain; which effect would cease if the pressure were removed, although in both these instances of the cessation of certain phenomena of disease upon the removal of an occasional cause, the assimilation upon which the disease itself depends may continue the same.

It appears then, that all these diseases are dependent upon an assimilating state of the principle, the phenomena of which are not dependent upon affection, whether by preternatural agents or by the re-agency of secondary disease. And the mode of discriminating the effects of secondary disease is, by the analytical test, by means which accomplish the removal of secondary disease, without previous influence upon the assimilating state

of the principle, or by analogy of circumstances to those mechanical ones on which such effects are known from collateral experience to depend.

The distinction between changes of products of the structures which are dependent upon the assimilation of the principle is made by the analytical inquiry, whether such change of structure or preternatural products could exist if the assimilating state of the principle were natural. I apprehend, that in cases of tumor, and in every form of disorganization, the dependence of these diseases is upon an assimilating state of the life, by which these effects on the structure were first produced; and without a continuance of the same, it appears impossible that they should be maintained. A material occasional cause, or one of affection, if not dependent on assimilation, is totally independent of the living state, exists by the force of its own causes, and would exist as well if removed from the animal as in connexion with his properties and his substances. The only examples of secondary disease, not dependent upon assimilation, which occur to me, are those of urinary and hepatic calculi, deposits of phosphate of lime, and, in short, substances which belong exclusively to the chemical and mechanical departments, and whose existence, though formed by relations of life with a fluid, some of the components of which belong to these departments, is nevertheless independent of life, which has, with respect to it, the importance only of a remote cause.

The relation of blood with the state of primary disease is one of the most obvious importance. In

the practice of medicine this relation is constantly presumed: there are few medical writings in which this relation is not remarked upon; but I am not aware that any very minute or satisfactory analysis of it has been attempted.

As primary disease is a modified state of life, and as every state of this principle is renewed from blood, so the supply of blood to a seat of disease is necessary to its existence and continuance. In this point of view, blood concurs only with the structures to maintain properties of life, to which these alliances are essential.

But we observe, that it is a common effect of disease to produce in its seats a disproportionate or increased quantity of blood. This effect of disease has been imputed by some to diminished tonic power in the vascular system, permitting an increased distention by the momentum of the blood. It has been imputed by others to a power of dilatation in the vascular system, by which the calibre of vessels is occasionally enlarged. This latter opinion I entertained, in common, I believe, with others, and expressed many years ago. But I now consider the evidence for both these explanations defective; and, in the chapter on determination, have endeavoured to connect the circumstance of an increased quantity of blood in the seats of disease with a power which acts unremittingly in aid of the heart in the business of the circulation. This power neither belongs to the tonicity of the arteries, (although their tonicity may be opposed to its exertion,) nor does it consist in one of dilatation; but has been said to be a power of derivation,

exerted by the affinity of life in the minute spheres of the structures, for the fluids which are separated by the arterial secerning extremities from blood by the force of this affinity. This power is not one of mere mechanical efficacy possessed by the vessels, and which, if one of dilatation, would only be perhaps a contraction of another set of fibres in an inverted order to that of those which diminish the area of the vessels; but the vessels, according to the solution before hazarded, obtain a preternatural quantity of blood, not because their area is first increased by another power, but because an increased quantity of blood is derived to them. In the one case, the increased quantity of blood in the vessels follows their dilatation; in the other, their dilatation is produced by the increased quantity of blood.

In the view before taken on this subject, the principal end of the exertion of the power of derivation, in health, has been said to be nutrition; or, that the fluids which maintain life and the structures should be separated from blood, and made to pervade the molecules of the structures, which are at once the seats of life and the subjects of renovation from these fluids. It is a common effect of disease to derive to its seat a preternatural quantity of the material which supports life; and as this can result only from an increased exertion of the ordinary power of derivation, so it seems probable that such increased exertion of the powers of life (or increased excitement, as it is termed,) is common in disease, and that such increased excitement, or more rapid consumption of life, re-

quires an additional or preternatural supply of the material which contains its elements in order to support it.

Thus disease is not only dependent upon blood in a physiological sense, or as blood is necessary to support life; but disease modifies or increases the supply of blood, to the end of supporting itself, or to the end of supporting this condition, or these circumstances of life. Thus far disease is dependent upon the concurrence of blood.

But, as primary disease may modify the structures, or lead to preternatural products which become re-agents, so the local effect of disease upon the circulation, the increased derivation of blood, may become a re-agent, and increase the energy of disease, or, in relation with it, modify its phenomena.

The increased derivation of blood to seats of disease is related also with the structures, as well as directly with the condition of life; and this relation furnishes a mediate one, by which life might be influenced by an agency proceeding from the presence of an increased quantity of blood. Thus, by increased vascular distention, spheres of related properties in the textures are disturbed and their phenomena modified; thus also vessels might be ruptured by the distention of blood, which they obtain by the exertion of a preternatural force of derivation.

The connexion then, of disease with blood, and its dependences on this fluid, may be thus enumerated:—

1. Life is maintained by assimilation from blood.

2. Life is related with the distribution of blood, and in its natural state produces a natural circulation; or, under the modification of disease, an increased, and probably sometimes a diminished, derivation of this fluid to its seat.

3. Primary disease, which belongs to the department of life, and assimilates, is maintained by the concurrence of blood, which furnishes the material of its renovation.

4. The increased derivation of blood depends, both in its degree and its continuance, upon the condition of life.

5. The dependence of the continuance of a disposition of life is upon assimilation.

6. A preternatural state of local circulation may have, with life, the relation of a cause; and the influence of this relation may be: first, to modify directly the condition of life, and consequently its phenomena; second, to influence the assimilation of life, and consequently the duration of disease, if the change produced on life by an influence from the circulation, is one which is capable of being in this way maintained; third, direct changes in the state of the structures may be produced by a change of the local circulation, which becomes thus mediately connected with subsequent phenomena, which are, according to the relation between life and the changes in the condition of the structures.

7. These relations between the parts of the constitution of disease obtain also in disease of primary and secondary seats; and the modes of influence between such seats is only a repetition of the order

and dependence exemplified by the constituents of disease in one seat.

8. As blood maintains disease by its concurrence, and as it is related with the state of disease in the department of life also, by affection, or is capable of influencing its condition; so the state of disease, whether with respect to the phenomena of affection or of assimilation, may be influenced by means whose direct agency is upon the circulation.

9. It is also a common effect of local disease to extend its influence directly by continuity of organic life, or through the medium of the nerves, with or without the participation of their centres, to the heart, by which this organ is made to concur in the supply of a preternatural quantity of blood to a seat of disease: and in this point of view, the action of the heart becomes very generally at once an indication of the degree and importance of local disease or irritation, and is also auxiliary to the power of derivation in the seat of disease, the energy of which is increased; and thus the action of the heart concurs in all the relations, both mechanical and vital, with the derivation of blood, and subserves to the increase, continuance, or modification of disease.

The continuance of disease, which is not maintained by a cause so far extraneous that, although perhaps an animal product, it is capable of existing independently of its connexion with the living structure, is dependent upon an assimilation of the diseased state in the department of life. Thus urinary and biliary calculi, extraneous substances, (as in cases of gun-shot wounds, &c.) maintain a

state of disease, which has no primary dependence upon assimilation.

The continuance of a preternatural state of the alliances of life, whether of blood or of the structures, is dependent upon an assimilation of that state of life which produces such variety in the circulation, or in the condition of the structures. Thus the continuance of inflammation, of tumors, of organic disease in general, depends upon the assimilation of a state of life, by which these conditions were first produced and are afterwards maintained.

As the state of life modifies the state of the organization with which it is allied, so the state of the organization, and more particularly that of the circulation, is related as a cause with life. This relation is termed one of re-agency. Thus it is an effect of disease to derive to its seat a preternatural quantity of blood, which, there is good reason for believing, increases in turn the energy of the disease, or, in some instances, is followed by ruptures of blood-vessels or other disorganization, which may increase the intensity of disease, or modify previous phenomena, or produce additional effects, or new symptoms, by new relations with the life of the same, or of distant seats.

The share in disease of re-agency of the change in the organization may be either to produce phenomena of affection, or to induce a change in the state of life, by which its assimilation may be influenced. Thus, in the former case, the irritation of a remote nerve, from the pressure of a tumor, may affect the brain, and occasion epilepsy. Thus

also, irritation in the intestinal canal may produce a similar effect by relation with seats whose life is only acted upon by a preternatural cause, and which was assimilated in such seats in a natural state, and displays no phenomena of disease, when such preternatural cause is removed. Thus, disorder may depend upon the presence of depraved secretions in the intestines; thus, the action of the heart may be disturbed during the temporary prevalence of an excited passion, &c. And thus, in the latter case, (a possibility which is rather in conformity with the laws of causation than demonstrated by any clear example) the presence of an increased quantity of blood, or the change of structure incident to organic disease, may so affect the life in its seat, or in a distant related one, as to produce a disease of assimilation, which will survive the re-action of the secondary disease. This seems to be illustrated in the case of white swelling, which produces hectic fever; which fever sometimes continues after the amputation of the limb. But in this, as in all other apparent examples, the inference is clear, that the continuance of disease is dependent upon the assimilation *in some seat;* but it is not clear, whether the change of the assimilating state was produced by the influence of a change of structure, or whether directly, by relation between the seats of properties of life.

The assimilation of life may be influenced by substituting one state of it for another, which substituted state will either continue the same assimilation, or will undergo changes which may exhibit other effects of the continued assimilation of a

diseased principle; or may terminate in the natural state of the assimilating principle. In the former case, the causation among the properties is at rest; in the latter, it is progressive, and these events depend upon the relations between the properties. The two results of an agency on the state of life, under disease, are exemplified both by the operation of medicines, and by related disease in different seats. Thus it may be the effect of medicine to modify the phenomena of disease, as long only as the influence of such medicine is continued; the former state of disease, together with its former phenomena, being resumed, when the influence of such medicine has ceased. A disease also, in a secondary seat, may modify the phenomena of a primary one: the secondary disease ceasing, the state of primary disease may be resumed, and maintained by assimilation. But either external preternatural agents, such as medicines, or a disease in a secondary seat, may be so related with the state of primary disease, as to affect it, and modify its phenomena; which phenomena continue to be displayed, after the cause of affection has ceased, if the change produced by a temporary action consists of a state which afterwards maintains itself by assimilation; or the action of a remedy, or of secondary disease, may be to dispose the properties engaged in primary disease to a progressive causation, which may consist in a series of assimilable states, and this series might end in one which displays permanently the same phenomena of disease; or it may end in the extinction of the principle, or a state of it in which

it is incapable of assimilating; or the natural or healthy state of the principle may be resumed.

It appears from this view, that primary disease consists of a change in the condition of life; that secondary related disease is an effect of this change; that the continuance of secondary disease is dependent upon the state which is assimilated in the department of life; that in the re-agency of secondary disease, this state has the operation of a cause which is related with primary disease, or may produce other effects, in relation with the organization; that the relation of secondary disease, as a cause with primary disease, is to modify its state, and consequently its phenomena; that the results of such modification of its state may be an affection of life, while its assimilation is unchanged; or an affection of life, which disposes it for the assimilation of another state; in the former case, the same phenomena of disease are continued, unless changes differently related are induced on the structures; in the latter case, the assimilating state may be changed by the influence of secondary disease, and the results of this change may be progressive disease, or a succession of assimilating states; or the series may end in an assimilating state, producing other phenomena of disease; or it may terminate in death, or recovery. These events depend upon relations between properties which are only inferred; and the events can only be anticipated, and that without certainty, by an experience of their connexion with the circumstances of the relation, which circumstances are denoted by signs, or symptoms.

The continuance of the same phenomena of disease is dependent upon the disposition of the causes concerned in the state of disease. The same disposition of disease is continued, when the causes concerned in its disposition are at rest; if varieties occur in the course of disease, it is either from a progressive causation, which has been before described, among the properties in the department of life, or from related influence, whether by external or preternatural agents, or from the re-action of secondary disease. If from the re-action of secondary disease, this disease is not that which is produced and maintained by a state of life which assimilates; but by progressive causation, arising out of the relations which belong to the parts of the organization; and the secondary disease has a mediate, instead of a direct, relation with the department of life.

5. It rarely happens that the constituents engaged in disease are at rest. There is most commonly a progressive causation going on, either among properties in the department of life, or else arising out of the relations between life, and organization, and externals. The common tendency of this progressive causation is, to resume the natural state, or to a recovery from the relations which originate phenomena of disease. In the course of the progressive causation, disease exhibits a diversity of symptoms, which correspond with the changes which its condition undergoes, and the recovery from a state of disease by these processes is regarded as the exertion of a vis mediatrix.

But this tendency of progressive change, in states

of disease, to recovery, although it may be said, perhaps, to prevail generally, is by no means universal; on the contrary, the tendency of successive change is frequently to death. Some forms of disease neither manifest a tendency to recovery nor death, but consist of states which exhibit permanently the same phenomena, and these phenomena continue until death takes place by the intervention of other disease, or from the operation of other causes. Diseases displaying neither the tendency to death, nor recovery, are exemplified in some cases of hemiplegia, or in more partial paralysis, as that of the retina in amaurosis; also in some tumors, which, although so far preternatural as to obtain the character of disease, are maintained through life, by provisions made for this purpose, as complete as those which support the natural structures; and are no more interested in the event of death than if they belonged to the natural or healthy condition of the subject.

As the living state is maintained by the relations between life and the organization, so this state might cease by a change in either of these departments. Certain deviations from a given state in either of these departments are compatible with the continuance of phenomena of life; but these phenomena are modified under such deviations, and in this way they are expressed: but other changes, in either department, frustrate wholly the object of their relation, and constitute a state which does not display modified phenomena of life, but is incompatible with their occurrence, or produce other effects.

The modes by which death occurs are either, 1st, by a change in the constitution of life, by which this principle no longer holds the relation with the structures from which the phenomena of the living state result; 2d, by changes in the organization, by which the concurrence of this system to maintain life is prevented; or by which an influence is exerted on life, which makes it a different form of existence, and consequently renders it incapable of the phenomena of its former state; or, 3d, by external agents, which disqualify the parts of this relation for a continuance of their former phenomena, whether by relation with life, or with organization, or with these directly or indirectly.

CHAPTER IX.

SPECULATIVE DOCTRINES OF THERAPEUTICS.

WHATEVER doctrine of disease has prevailed, the same remedies have very generally been resorted to: the means were the same; the explanations connected with them only different. The employment of remedies has indeed been modified, according to the doctrines of pathology which have prevailed at different times; but whether under the supposition of solar or of lunar influence, of hot or cold humours, bleedings, emetics, purgatives, sudorifics, &c., have been employed, from almost the earliest periods of medicine. The remedies of the Galenists were still employed by the chemists; and the mercury and opium of Paracelsus have held an important rank in the materia medica of succeeding systematics.

The principal changes which the materia medica has undergone in the lapse of ages are the improvements for which it is indebted to experience. Agreeably with this test, efficient remedies have been retained, new ones adopted, or abandoned, under every form of pathology, and the inert ones, which have enjoyed a temporary credit, have been rejected, upon a conviction of their inefficacy. Thus, experience has, to a great extent, still directed the career of medicine; and theory has busied itself rather in forcing a conformity between facts and doctrines, than in sug-

gesting means by which certain ends might be anticipated or attained.

If theory had always been content to follow experience, perhaps, upon the whole, much mischief might have been avoided; but although it has been the business of systematics to reconcile their doctrines with experience, yet a considerable share has rested, and still must rest, upon theory; and this share properly consists in supplying the deficiencies of experience; and in directing by precept the efforts of art, on occasions where the voice of experience is either wholly silent, or, when her testimonies fall short of the purposes and scope, which could scarcely be attempted without a guidance of some sort. Thus, theory has been of necessity the ally of experience; and overlooking her proper subordination, has sometimes, by assuming the chief command, lent her services to the enemy; and instead of modestly following experience, or endeavouring, by a conformity with her indications, to supply her deficiencies, has mistaken her banners, and acted in opposition to her counsels.

Previously to the introduction of the mechanical philosophy into medicine, it is difficult to say with what view remedies were prescribed; but it would appear from the formulæ, and combinations of means, in which the productions of the vegetable and mineral kingdoms, together with those of the animal creation, were jumbled together, that the curative properties of substances were thought to be tolerably universal; that they were supposed respectively to possess their particular virtues; and that a heterogeneous combination of substances

in all the departments of nature would afford at once the double advantage of bringing many means to bear upon the same end, and of securing a multitude of chances, in the reasonable expectation that if some failed, others might succeed. A plan of therapeutics, thus directed, it is very obvious, must leave a vast deal to accident.

But scarcely more reasonable were the views of the mechanical physicians. Their pathology was founded upon the varieties of shapes in fluid particles, the tenuity or inspissation of fluids; the adaptations of particles to tubes, or of tubes to particles, large or small, round or angular. It is scarcely necessary to remark further upon a system of therapeutics which aspired to correct the shapes and cohesions of particles, to dilute some fluids, and to sheath the acrimony of others; to introduce round particles, where those were in excess which abounded in angles and asperities; to render spicula inoffensive, or to quicken the movements of fluids which were disposed to remora: it is unnecessary to remark further upon such laudable intentions, than that it may sometimes be difficult to know precisely in what the depravity or deformity of fluid particles consisted; and hence, that in the administration of correctives, there was great liability to mistakes.

The pathology of Boerhaave was, in its design, more closely allied with the business of cure than that to which it gave place. A remedial treatment was certainly suggested by a doctrine which taught that diseases were produced by particles of erroneous shapes, which, being well defined,

nothing remained to be done but to correct them. This doctrine certainly proposed a treatment; but the one that succeeded to it, although the result of an advance in the understanding of the subject, was rather calculated to produce the rejection of former remedies, than to suggest any very clear notions with respect to the employment either of new ones, or of such old ones as were retained.

In addition, therefore, to the mechanical constitution of an animal, and as an improvement upon the mechanical pathology, there was a vague recommendation to take also his vital properties into the account. But as these properties were at that time an entirely new acquaintance, there was scarcely an imagination hazarded on the manner in which it would be most agreeable to them to be treated. It seemed to be a general opinion that they were rather fond of cordials; in the gratification of which taste, the rules of sobriety, which should have been observed, were not unfrequently transgressed. The bleedings, purgatives, emetics, sudorifics, &c. which had kept their ground through all previous conflicts of opinion, were also still resorted to: it was supposed that the properties of life had some sort of connexion with these agents; and accordingly an intimacy was forced between them, rather on the supposition of some previous distant relationship, than by a regular introduction.

That these vital properties were not to be overlooked, was strenuously insisted upon by Cullen: but it never appeared that his information with

respect to these properties extended much farther than the confession that they were real entities, though belonging rather to the spiritual than the material world. While the existence and importance of these properties were strenuously insisted upon by Cullen, and embodied in his pathology, his practice was founded upon experience; and the only therapeutical design which arose clearly out of his pathology was perhaps to relax a spasm, which, it is more than probable, did not exist.

The connexion was sufficiently intimate between the pathology and the therapeutics of Brown. The principles of disease, according to his system, directed the operations of cure. But as his pathology was, as we have seen, essentially erroneous, so it was much more mischievous than if it had, like some former ones, aspired only to the explanation of facts: the business of cure would then have been rested upon experience: a direction which would have been much more safely confided in than that proceeding from false principles.

The pathology of Brown supposed only two states in disease (with their modifications); one, of debility, and the other, of excess of strength. This hypothesis, in addition to its being totally inadequate to explain the phenomena of disease, was still more grossly defective in regard to the system of cure which it inculcated. Diseases were said to consist in certain states, of which there was no criterion; and a treatment was consequently proposed, the object of which was to correct states, which, in addition to their being perhaps altogether falsely inferred, could be recognised by no rule.

A great range of febrile and inflammatory disease was supposed to be dependent on debility; while phenomena of the same class, whose character was similar, and not distinguished in the doctrine by any defined criterion, were supposed to depend upon excess of vigour. The absence of just distinction, of course, left it open for caprice to decide to which of these states a disease belonged. A selection so governed could scarcely be expected to be more frequently right than wrong; and as the remedies employed were of no weak description, and the common guidance of experience but little respected, it could not fail to happen that a pathology, at once visionary, bold, and capricious, should be productive of many mistakes, and of much evil.

Febrile and inflammatory diseases were among the most conspicuous that were treated upon these new principles: the success of a stimulating treatment, in some forms of inflammation, induced an adoption of this plan in other instances, almost without limit, and certainly without much discrimination. It accordingly happened that the means which cured in some instances were fatal in others. The cure of inflammatory diseases may sometimes be accomplished by stimuli; but the true principle was overlooked: their cure also, as was well known, might be accomplished by an opposite treatment; and this experience alone should have sufficed to show that the principle was false which assumed a state in disease which was to be remedied only by one class of means: it should have suggested a theory at least in agree-

ment with the experience, which would admit the possibility of the same end being attained by different, or even by opposite means.

If, as is frequently proved by our experience, states of disease which resemble each other may be cured by opposite means, a principle is at least defective, according to which only one treatment can be appropriate or successful. But to say nothing about the error or deficiency of the principle, supposing the fact to be confessed that the same end may be attained by different or even opposite means, it becomes an object of great importance to distinguish, in the several examples, which of these means is the most safe, or the least objectionable, as well as the most certain in the attainment of the end. It is probable that violent stimulation would frequently cure the diseases which are otherwise cured by depletion, provided the effect of this plan were not fatal before the object was attained; which object is the general one of all treatments, except such as are instituted to remove known causes, namely, the subversion of a state of disease. But it was the error of the system of Brown to adopt this plan of stimulation without regard to experience, which is the only test by which to decide, of two plans, which is the best. He found that certain febrile states, without visceral disease, and certain states of inflammation in the extremities, may be successfully treated by stimuli; and generalizing, at once, the dependence of fever and inflammation on debility, and also the treatment of these states by stimuli, he employed the same means in fevers connected

with visceral disease, and also in idiopathic visceral inflammations, as those which had succeeded in cases bearing a similitude only in the least essential respects. The treatment of some forms of inflammation, in safe seats, by stimuli, may perhaps sometimes be hazarded with impunity and success: but if inflammation, of similar degree, existed in an organ essential or important to life, the exacerbation produced by stimuli may be fatal before the subversion of a diseased state is produced, which may be followed by recovery.

Thus fevers accompanied perhaps by those which are considered the signs of the most extreme debility, commonly involve an affection of the brain, which is little short of phrenitis: it must have occurred to many practitioners to have seen the use of stimuli in these states followed by accelerated action of the heart, delirium, perhaps sudden insensibility, with convulsions, terminating in death, or the more gradual occurrence of effusion, coma, and death.

Although the general tendency of any preternatural agent which is related with disease may be to cure, by subverting, this state; yet the causes of disease which succeed to the employment of preternatural agents, respectively, are very different. Exacerbation of disease is the frequent, perhaps the common effect of stimuli: the possible extent to which this exacerbation may proceed is by no means defined, and can in very few instances be anticipated. Hence this treatment is unsafe: and the more especially objectionable, as the same end may in general be accomplished

by means which, instead of exacerbating symptoms, preparatory to the cure, diminish the intensity of disease by their precursory operation, and bring it within safe bounds.

Resting, therefore, the therapeutics suggested by the system of Brown upon the testimony of experience, they may be said to be generally unsafe: so far as they are connected with his pathology, they must be erroneous, because the pathology itself is false: and inasmuch as this pathology inculcates a general, or almost universal, adoption of unsafe measures upon erroneous inferences, the practice, in connexion with the principle, must be very generally pernicious.

It is not intended, by these objections, to make an universal condemnation of the treatment of diseases by stimuli: this would be as unwise as the deduction of any other general rules, which are contrary to our experience. It is merely intended to insist upon the general propriety of selecting the safest means, where there appears a possibility of attaining the same ends by different methods. We are taught only by experience the instances in which this choice of means is permitted us: there are many cases in which the employment of stimuli is sanctioned by this best instructor: but they rarely belong to the class of such febrile or inflammatory disorders as prevail in this country; and when, in these disorders, the more judicious means have failed, experience still teaches us, that the employment of stimuli in any form is to be ventured upon with caution, and pursued with a strict observation of their effects.

It must of course happen, that, so far as a doctrine of cure is dependent upon a doctrine of disease, if the latter is erroneous, the former cannot be correct. We have, in our examination of that subject, seen reason to doubt, or rather to reject the pathology which ascribes all diseases to the presence of a preternatural quantity of blood, in some part or parts. We have seen, that the state of disease precedes the possession of a disproportionate quantity of blood in an organ; that the excess of blood in any seat is produced by disease; that the dependences of the circulation are so numerous, that many, or even opposite states, in the governing principle, affecting different orders of vessels, may produce this consequence, which is therefore a common one of disease; that such excess of blood may increase disease, or modify its phenomena: but that it is only an accompaniment, or a re-agent of disease; that, being first produced by disease, its continuance is dependent upon that of disease.

The practice of medicine would indeed be one of great simplicity, and very soon learned, if it consisted only in the pathological doctrine that all disease is produced by local excess of blood; and that the mode of cure is to diminish the quantity of this fluid, until the disease ceases. So universal a pathology can be founded only on false principles; which principles can be inferred only from a negligent or imperfect analysis of the departments of an animal constitution, and their relations with each other. Something like an analysis on this subject has here been attempted, pro-

bably with very partial success: but being the only one which, I believe, ever has been attempted to an equal extent, its results must be admitted, until it is substituted by a better.

According to this analysis, the presence of an increased quantity of blood in a seat is produced by disease: the state of disease is maintained by blood, as by the concurrence of the structures, because blood is necessary to life, of which disease is a condition. The relation of blood with disease, in this view, is merely that of a concurring cause: but the presence of an increased quantity of blood in a part, may also have with disease the relation of a re-agent, and may, in this quality, increase its degree, or modify its phenomena, by direct or indirect relations before specified. Thus it appears that disease is no further necessarily dependent upon blood, than that this fluid is necessary to life, and to some of the appearances, of the hydraulic kind, which are exhibited by disease. The cure, therefore, of diseases by abstracting blood, can have no reference, in general, to removing the cause of a disease; it may reduce the quantity of that which is a supporter of the diseased state, or with regard to the phenomena of re-agency, it may diminish an actual cause of disease; but, in general, it can have no direct curative relation with the state of disease, which relation is that of removing a cause upon which an effect depends.

Hence it appears that the practice of curing diseases by the abstraction of blood can scarcely ever entertain for its object the design of removing

in this way the cause of such diseases; for the state of disease compels the preternatural determination of blood; and if this preternatural quantity of blood in a seat is not the cause of the disease, the dependence of the cure of determination itself is, not upon the abstraction of blood, which cannot be wholly withdrawn without universal death, but upon a relation with disease of another kind, by which disease first ceases, and the restoration of the natural circulation succeeds.

Although it is admitted, in our former analysis, that the change produced in the circulation by local disease may have the influence of a re-agent, in which quality it may either increase or modify disease; yet, I presume, it is scarcely possible to discriminate between the phenomena which depend on this re-agency, and those which might be produced by the state of disease, in either of the allied departments. It is also clear that the abstraction of blood cannot be recommended with a view to the removal of the cause of disease (except in relation with a predisposition) on the ground that it is a supporter, and so far a cause of disease: in this point of view, the dependence of life and of disease on blood are equal; and the terms of curing the latter state by the abstraction of a cause, are those also of the extinction of life.

The phenomena which appear to have the most simple dependence upon fulness of blood are those which cease frequently after one or two bleedings, as in some affections of the head, with accompanying signs of plethora. In these cases, if the disease were assumed independently of blood, the

abstraction of this fluid might be curative by the common mode of operation of other remedies: but it is probable that the local change has not gone into the state of disease, but is one only of a predisposition, which is excitable by the presence of a certain quantity of blood; and therefore the disease, which is the result of this relation, may be cured by bleeding, as by the removal of a concurring cause, or one which excites a local predisposition.

We observe that many diseases, which are perhaps successfully treated by bleeding, get well under different, and sometimes opposite remedies; and that sometimes, when their course is left wholly to nature, they get well without any remedies at all: and during the spontaneous cure, nature is so far from adopting invariably processes analogous in their effect to artificial depletion, that I have known chronic disease of the head cease gradually as the plethora of the system was increased; an example which is by no means rare. The curative efficacy therefore of the abstraction of blood appears to be not peculiar to this means, but common to other agents: the reason of which is, that bleeding does not remove a precise or identical cause (except in the few instances, in which a certain degree of vascular fulness may excite a local predisposition), but influences this state of disease by a relation which is common to many other preternatural agents. This common relation furnishes a common principle of cure, which has already been sometimes hinted, and will presently be more fully detailed.

The more sensible phenomena of that which is called determination of blood depend upon the continuance of disease. Under the most violent depletion, we may observe that determination of blood, whether simple or inflammatory, in the particular seats of disease, continues undiminished. All other parts of the constitution seem to suffer, or to be affected by repeated bleedings, except the seat of the disease: and although the actual quantity of blood, in the system of the circulation, is of course diminished, the relative disproportion is unabated.

Hence the dependence of cure by the abstraction of blood is on the relation between this change in the circulation, and the state of disease. In this respect, bleeding exemplifies only the dependence of cure which is common to all other remedies, and is liable, like other remedies, to the opposite relation with disease, namely, that of augmenting its degree, if the relation supersedes the tendency to reduction which belongs to a diminution of a supporter of life; and of modifying its phenomena; of changing its form; and of perpetuating its state. With the continuance of disease the determination of blood continues; and is a forced, or dependent accompaniment; and therefore bleeding will not even cure the determination of blood, unless it has a curative relation with the state of disease. The only dependences of disease on blood, are, 1st, *on blood as a supporter both of disease and of life, of which disease is a modification;* 2d, *on blood as an exciting cause in relation with a local predisposition;* 3d, *on blood as a concurring cause or re-agent.*

The curative efficacy, therefore, of bleeding is the result of a certain relation of this sort with the state of disease: but this relation can no more belong universally to the remedy, than any other one agent can have the same relation with a diversity of states. If the curative relation of bleeding with disease is not universal, we judge of the instances in which this relation subsists, as of all other relations, by experience: and the only *à priori* principle in the direction of bleeding as a means of cure, is a diminution of that which is either an exciting cause, or a supporter of disease. In what instances this measure, or any other, holds the curative relation with disease must be inferred from experience, or an observation of the results of the employment of this measure, in connexion with certain signs: and our selection of this remedy, in the several examples, will be in conformity with the instructions derived from this source.

As it has been seen in our pathology that disease has by no means a common origin in disorder of the digestive organs (or any other set of organs), so the therapeutics which are founded upon the principle of correcting disorder which does not exist, or which entertains the design of curing diseases of supposed dependence, which acknowledge no such relation, must of course be false.

It has appeared in our former analysis, that the relation of disorder of the digestive organs, with disease in other seats, is that either of an effect, a cause, or a re-agent. Hence it is obvious, that the cure of disorder of the digestive organs is capable of

remedying disorder in other seats, only of one kind, by the removal of a cause which produces and maintains disease; and this kind is that class of secondary disorders which are dependent upon a primary one of the digestive organs. But it has been seen that there is, in general, great difficulty in discriminating what disorders in other seats are dependent upon disorder of the digestive organs: so difficult is this discrimination, that, in the instances of this dependence, which are currently supposed, there is often more reason to think the disorder of the digestive organs an effect, than a cause of disorder in other seats; and if neither of these alternatives should be declared by the order of succession, the disorders of different seats will appear to be concomitant, and sometimes unconnected.

As the principle of an universal dependence of disease in other seats upon that of the digestive organs is false, so the design of treating diseases upon the presumption of this universal dependence must be erroneous. As the instances of disorder in other seats, connected with that of the digestive organs, exemplify three relations with their subdivisions, and as these relations can rarely be discriminated, and when they admit of being discriminated, that of dependence of other disease upon disorder of the digestive organs is perhaps of the least frequent occurrence; so remedies can scarcely ever be directed with the precise views of an ascertained relation; and if ever exhibited upon a presumption of such relation, their employment must be very limited.

But we find that means whose operation is on the digestive organs, however equivocal the relation of the state of these organs with other disease might be, frequently cure disorder in other seats, whether it is related with that of the digestive organs as a cause, an effect, or a re-agent. Hence, as the cure in these cases is not according to the view with which the means were instituted, or is not in the way of direct causation, so such curative results must depend on a modus operandi which is not contemplated in this defective scheme of therapeutics, and will be found hereafter to illustrate a more general principle, which is conformable with the whole of our experience. Indeed, the employment of means for the cure of disorder of the digestive organs is no more precise than the view of curing other disorder by supposed dependence of it upon disorder of the digestive organs; for as it is not known in what the deviation from health under disease of these organs consists, so the remedies are not exhibited with a view to supply deficient causes, or to remove those which are preternatural, or to modify combinations among properties which cannot be recognized, but upon some general principle, as that on which other remedies act, which do not remove by an unmixed agency known material causes; namely, by a process of indirect causation.

The doctrine, that all diseases have a local origin, we have found to be defective in proof. It has appeared that there is, to a great extent, in diseases, a diathesis which is apparently constitutional; at least, involving disorder of as many

functions and seats as from sympathy or predisposition are related with the exciting causes; and that, in connexion with such general state of disorder, and often succeeding to its signs, disease appears to affect particularly, or in a greater degree, one viscus or system. This order is, however, by no means universal; the origin of disease in one seat, on other occasions, is both agreeable with our experience and preconceptions; and it is deducible from the constitution of structures, as well as agreeable with experience, that no one seat has the exclusive power of originating disease; and consequently, that none is exempt from the liability to commence its processes.

That the local origin of disease consists of an undue or preternatural distribution of nervous energy, has been seen to be not only a mere assumption, but one which is not agreeable with the known physiology of the nervous system; inadequate to explain the phenomena of disease, and incompatible with other conclusions which are better supported. The pathology, thus defective, may, however, give rise to a doctrine of therapeutics which, so far as it depends upon the pathology, can scarcely boast a superior quality. Nevertheless, a practice in connexion with these, as with most other doctrines, might be successful, upon a common law of agreement in the agency of remedies, with whatever contradictory views they may be directed.

If disease consists in a local excess of nervous energy, the diminution of this energy will, of course, constitute its cure. But it is to be asked,

why did this excess of energy in a part take place? The answer to this question will imply a causation of preparatory disease, without going into more elaborate proofs, which preceded and produced the alleged cause of disease. But it is proved by the common phenomena of disease, that this is a state which is made up of very complex relations between the causes of several departments; and that, in many of its forms, not one of the phenomena can be clearly referred to any variety of nervous energy, or even to the participation of this power in any degree.

The therapeutical design, connected with a pathology which assigns, generally, a local excess of nervous energy as the cause of disease, it has been said, is to diminish this energy, or make its distribution more equal. The means resorted to for this purpose are the common instruments of therapeutics which, as before mentioned, are sanctioned by experience, and have been successful almost from the earliest periods of medicine, however various or conflicting the opinions with which the use of these means might have been connected.

To diminish local excess of nervous energy, bleedings might be employed, upon the principle of reducing the pabulum, or supporter of nervous energy; emetics, purgatives, blisters, and all other means of counter-irritation to diminish nervous energy in one seat, by deriving it to another. The practice, therefore, is the common one which is approved by experience; and so far it may be right: but with respect to the therapeutical design, it is scarcely necessary to say more than that, although

the practice may be right, the principle of its institution, if founded upon false inferences, must be wrong.

Indeed, this pathology has the merit, which is no trifling one, of suggesting very little more than the common remedies, and the common mode of employing them. There is, however, one therapeutical design which is more closely connected with the physiology on which the doctrine should rest than any other, the practical results of which would perhaps not be especially happy. We know that the effects of ordinary remedies are not confined to their relations with one department of the living constitution: the abstraction of blood produces in the system hydraulic changes; the other evacuant remedies increase the secretions of particular glands, or dispose secreting surfaces to pour out mucus, or lymph, or serum, to relieve parts from the operation of intestinal accumulations, &c. By these agencies, the operation of ordinary remedies is perhaps rather complicated. But a more palpable indication for diminishing nervous energy in one seat, is to consume almost the only nervous energy which is recognised in another. Thus, if a person has inflammation of the brain, liver, or stomach, a cancerous wound, &c., he cannot more effectually make a diversion of energy, purely nervous, than by a determined exertion of the voluntary muscles. Instead, therefore, of bleeding, purgatives, blisters, and all the indirect remedies, the prescription the most forcibly recommended by the doctrine in a case of hepatitis or consumption, for example, would be, that the

patient should exercise himself for a few hours in the tread-mill; or, that one labouring under phrenitis, or pneumonia, should be made to walk fifteen or twenty miles a day; by which it would be presumed so much nervous energy would be consumed in the legs and arms that there could not possibly be any preponderance or excess in any other seat: at all events, so effectual a derivation of this principle to the extensive muscular systems of these parts could not fail to diminish it elsewhere; a design which in other cases may have given the colour of a principle to the medicina gymnastica.

The doctrine that *disease* is salutary, or that disease is curative, has arisen out of a cursory observation of relations, or rather, it is a remark which has not been subjected to analytical examination. In the chapter on related disease, it has been shown, that the cure of a disease depends upon a process of causation among related agents. In this process of causation, secondary or additional disease frequently occurs, and the primary disease may cease in consequence; but this happens, not because the causation is one of disease, or exhibits other phenomena of disease, but because the phenomena occur in consequence of changes which lead to the recovery of health.

Secondary disease is very frequently exhibited in a spontaneous curative process; but the cure does not happen because the process is disease, but because it is one tending to recovery. If a curative process could not go on in states of disease without additional disease, there would be no ter-

mination of disease. But the fact is otherwise: primary disease may cease without secondary, and secondary without additional disease in another seat; so that the dependence of the recovery is upon curative relations; and the stages of change, which exhibit additional disease, are so far from always holding this relation with primary disease, that a series of change which tends to death by a long and complicated course of disease may exhibit the phenomena of secondary or more remote additional disease, without curing the primary disease; or even the primary disease may be exacerbated by every occurrence of additional disease.

Hence, in spontaneous processes, the dependence of cure is not upon the disease, but upon a causation, the series of which terminates in this event; in the progress of which causation, the form of the disease, as well as its seat, may be many times changed; or this causation may constitute a process of recovery which will be progressive to its termination, without exhibiting symptoms of new or additional disease.

That which is true of the pathology, must be true also of the therapeutics which are founded upon the pathology. As the pathology proposes no terms for the final cessation of disease; or rather, according to the pathology, disease would never cease; so a doctrine, so contrary to experience, cannot be otherwise than erroneous. It nevertheless suggests an easy scheme of therapeutics, and one which has blended in it the truths of experience with the errors of defective observation.

It recommends the production of artificial disease to cure a natural one. It is very obvious that, if to produce artificial disease was all that was required in the business of cure, we should meet with invariable success in our undertakings with this view; it is always in our power to produce *disease* by remedies: but as the original disease very frequently continues notwithstanding, or proceeds to a fatal termination, so it may be guessed that additional disease does not constitute the terms of cure, but that recovery depends upon *curative* relations, whether of artificial, or spontaneous influences, with the state of disease.

If then a remedy which is curative produces other disease of its own, preparatory to this event, the remedy cures, not because it produces disease, but because it originates a causation which terminates in recovery; and the disease produced by the remedy at once illustrates a course of causation, and furnishes a sign by which the event may be anticipated, according to previous experience of the connexion between states which are denoted by such sign, and the events of recovery or death. It would indeed be a great error to suppose that cure depends on disease. The contrary is witnessed in our daily practice. We have a hundred means of producing disease; and if this were all that was required for the cure of disease, we need not trouble ourselves about the selection; for one remedy which produces disease is as good as another. But the fact is otherwise: it is not that our remedies must produce disease, but that they must entertain curative relations; in the processes

resulting from which, other disease may or may not be exhibited, in order to accomplish the end for which they were employed.

Mr. Hunter expressed a doctrine very similar to the one just discussed, though his view of it was rather more philosophical. He did not generalize the observation that secondary was curative of primary disease; but he thought that diseases were cured by making one disordered action substitute another. This doctrine, like the last, is resting the dependence of cure on disease. It has been shown, that additional disease in the curative operations of remedies is an accidental, and not an essential occurrence: the cure depends not upon disease, but upon a progressive change in the properties concerned in disease, and in their relations with each other, by which this termination is accomplished. This doctrine, therefore, is defective, inasmuch as it proposes an essential dependence upon that which is little more than an accident; and erroneous, by making that an efficient cause which is only a sign of a process, or of a causation. The doctrine is still further defective, by its proposing a dependence of the cure of disease upon other disease; by which dependence, if it were true, the state of disease could never cease, and a man's life might be passed in suffering under variety of disease, or an endless succession of diseased states, of which the terms of the cessation of one form, are the supervention of another. It may, for the sake of retaining accustomed medical language, be said with propriety on this subject, that the cure of disease does not depend

merely upon other disease, but that there are some forms of disease which will not change spontaneously to the end of recovery; and that there are others in which the vis medicatrix tends to produce this event; that the cure of the former may therefore be accomplished by substituting the latter.

The following brief abstract from our previous discussions may serve to connect the speculative principles of therapeutics with those of pathology:—

1. The state of disease is a change in the constitution of health.

2. The constitution of health is made by the union and reciprocal influence of properties of life and organization.

3. The change which constitutes disease is sometimes manifested to the senses when it belongs to the organization, or to the hydraulic department, and we are able to define in what this change consists. But as the state of disease is never wholly constituted by a change in this department, but is preceded by one of life, which is not an object of the senses; so we can obtain only a partial testimony, of the sensible kind, of that in which the deviation from health consists.

4. Primitive spontaneous disease affects properties in the department of life, which develop disease by predispositions, with or without the aid of exciting causes.

5. Secondary disease belongs either to the seat of primary disease, and consists of the effects of primary disease on its alliances, (blood, and the

structures, &c.) or else, secondary disease is exemplified by its occurrence in distant seats.

6. Secondary may re-act upon primary disease, whether in its own seat or occupying related seats, and produce other disease or modify its phenomena.

7. Disease which is characterized by phenomena dependent upon life, is maintained by assimilation in the department of life.

8. The material effects of primary disease; viz. the changes in the hydraulic or mechanical condition of the structures, which, having once occurred, are not dependent for their support upon life, might maintain a state of disease by affection independently of assimilation. Of these examples the least equivocal are certain concretions, calculous formations, &c.

9. Disease may be maintained by the effects of primary disease which are dependent upon assimilation, also by affection. Or, assimilating disease may be induced by the causes of affection. But the causes of affection are, in these instances, themselves dependent upon assimilation.

10. As long as a diseased assimilation can be maintained, disease will be maintained. The cessation of disease requires a change, by which the diseased state is no longer assimilated, and then the healthy one may be resumed.

As it is impossible to specify the state of disease, or to say in what the deviation from health consists, for the reason, that the properties engaged in disease, which are declared by their effects to be

very numerous, are not objects of the senses; so it must be equally impossible to institute a treatment upon a direct principle of causation, which is that of conferring deficient properties, or removing those which are heterogeneous or foreign. Hence, if remedies supplied the precise causes which would correct the deviation from health, we could not administer them with this view, because it cannot be known in what the deviation from health consists.

Nevertheless, although we cannot aspire to this sure method of causation in reference to the entire state of disease, or in cases in which an inscrutable deviation from a given state in the department of life originates, and still maintains disease; yet, as the sensible and inferred causes are generally mixed in the conditions of disease, the treatment, on a direct principle of causation, may sometimes entertain partial relations with the constitution of disease. These relations are, of course, with the sensible modifications of the organization and its products. But, as the state of disease is a complicated one, so also are the relations by which remedies exert their influence upon it. It has been the attempt of the preceding pages to disentangle this knot, and to make an inferential analysis of the agents and modes of disease.—The following therapeutical analysis being (as all doctrines of therapeutics must) founded on the principles of disease, is at once deduced from the preceding pathology, and illustrates its truth by its agreement with the general results of experience.

1. Those remedies only act upon a direct principle of causation, which remove known material causes. This curative process has reference only to the effects of primary disease, and no other phenomena of disease are made to cease by such process, except such as depend upon the re-action of the effects of previous disease. Thus, when other previous disease has ceased, the bowels may be loaded with depraved secretions, the presence of which might produce disorder, which will cease on the removal of this material cause, or re-agent: the action of a purgative in this case may be, to some extent, upon the principle of direct causation.

2. But curative remedies, in general, act by indirect relations with disease, which dispose this state for a change, which terminates in health. Thus, dyspepsia may be cured sometimes by long continued exhibition of ipecacuanha, in small doses; or the disposition to unhealthy abdominal secretions may be subverted by the long continued, reiterated action of small doses of blue pill; or inflammations, as of the eye, erysipelatous inflammations, or some glandular inflammations; or the disposition to accelerated action in the powers which animate the heart, or catarrhal affection of the bronchia; or some forms of rheumatism, or some forms of dropsy, &c., may be cured by reiterating the influence of nauseating or emetic remedies; or, the same diseases may otherwise cease, under a course of depletion, by the lancet, &c. The relation of the remedy with the disease is here indirect: it

cannot be employed with the view of removing or supplying causes which are not known; and can only be conjectured in a superficial manner.

3. No closer or more precise connexion then can in general be defined between remedies and disease, than that the former are, with respect to the latter, related causes or influences. This relation between remedies and disease may be productive of two results: 1st, remedies may affect disease only so long as they are supplied; which is either suspended, or else the remedies produce phenomena in conjunction with disease; or, 2d, they may change the assimilation, by which the state of disease is maintained. The first are exemplified by narcotics, as opium, &c., and by those remedies which make a temporary impression on disease, the former state of which is perfectly resumed, as soon as the operation of the remedy has passed. The second are exemplified by those remedies which permanently reduce the intensity of symptoms, or which produce changes in the state of disease which remain, and are characterised by symptoms, when the remedy has been for some time discontinued. To this class belong all those remedies which, without curing disease under their exhibition, change the form and character of the original disease. There is scarcely a remedy which, in relation with some form of disease, does not exemplify this result; which may be remarked alike of bleedings, emetics, purgatives, issues, mercury, arsenic, sulphur, hemlock, &c., as well as of those means which, in the more transient diseases of action, are more suddenly followed by recovery of

health, as stimuli, antispasmodics, &c., as they are called.

4. The results of a change in the assimilation of disease are two: 1st, either the substitution of another permanent assimilating state; or, 2d, a progressive change in the state of disease. This progressive change arises from new relations among the constituents of seats, by which changes are produced, or states rest, according to these relations. The first is exemplified by the effects of repeated bleedings (and of many other means) which are followed either by the recovery of health, or by the supervention of another assimilating state of disease, as of dropsy. The second, or progressive change by relation of a remedy with a disease, is exemplified by the exhibition of mercury (as well as by many other means), which substitutes a new for a previous diseased state; and this new state, by progressive change, or a succession of disease, goes through the range from ordinary mercurial affection, anomalous fever, variety of irregular, local, and constitutional states, to, perhaps, consumption and death.

5. The effects of the subversion of one assimilating state, and the progressive changes which succeed, are three: 1st, the assumption of a settled state of disease, after a series of phenomena characteristic of a succession of states; 2d, the cessation of an assimilating state of disease, and the resumption of the healthy assimilating state; or, 3d, the cessation of the assimilating state of disease, together with that of the principle of life. The first is exemplified in the occurrence of per-

manent substituted disease: the second, in recovery after a course of disease, the successive states of which are characterized by symptoms; the third, is the termination of progressive change in death.

The classes of remedies are the following:—

1. Those which have direct, and,

2. Those which have indirect, relation with the state of life. The agency of these being generally mixed, it is seldom that the precise effects of either, on any one department of constitution, can be discriminated. They may, however, be exemplified; the first, by opium, hemlock, arsenic, &c.; the second, by such agents as have hydraulic or mechanical relation with blood and the structures: these relate exclusively to means whose primary operation is of the mechanical kind, as pressure, tension, &c.; and the mixed operation of these modes is exemplified by emetics, purgatives, diuretics, &c., which, among other modes of influence, appear to act by a revulsion, or derivation of fluids from the seats of disease to related ones.

3. Those remedies which act on disease by employment on its seat, directly or indirectly. To this class belong the topical remedies, and those internal ones which may be supposed to act on seats of internal disease, either by immediate application to them, if they belong to those cavities which have external openings, as the stomach, rectum, urethra, and bladder; or by an application to seats of disease through the medium of the circulation. The agency in this latter case is, on some occasions, in which it is commonly supposed, perhaps rather equivocal: effects on disease through

the medium of the circulation are most commonly indirect, as by the influence of remedies on related organs, as the brain, liver, heart, skin, &c.; when these parts are themselves the seats of disease, it is not improbable but the operation of remedies upon them, through the circulation, might exemplify this mode.

4. Those remedies which act on disease by a primary operation on a related seat, directly or indirectly. This class of remedies is by far the most numerous: indeed there are very few remedies, and these are, perhaps, rather ambiguous, which this class does not comprehend. The different modes of bleeding exemplify, in general, the employment of a remedy on a related seat, whether by cupping, leeches, or by the lancet. To this class belong also all those remedies whose relation is with the stomach, bowels, with the glandular system, and the skin; such as ipecacuanha, antimony, nitre, squill, digitalis, all the various purgative medicines, mercury, &c. Issues, blisters, &c., are also employed, upon the presumption of this relation. The sensible effects of remedies employed upon related seats are most commonly those of derivation. This mode of operation is strikingly illustrated under the old term, revulsion. The mode of the operation of this extensive class of remedies, stating only their sensible effects, would be said to be that of derivation of fluids from seats of disease, to others, in consequence of artificial stimulation. The cure of the disease is thus supposed to be accomplished merely by a diminution of the fluids in its seat. The removal of the pres-

sure of the atmosphere from certain parts, as the feet, &c., thereby permitting an increased circulation in such parts by means of the air-pump bath, as it is termed, is proposed on a similar principle.

We have seen, in our former pathological examinations, that the presence of a preternatural quantity of fluids in the seats of disease, is the effect of a primary disease, or of a previous change in the powers by which the quantity of fluids is determined, independently of any primary variation in the contractilities which limit the area of the vascular system. The necessary relation of the fluids of a seat with the state of disease which occupies it, is that of a supporter, or concurrent cause; but the state of disease continuing, however considerable the depletion might be, or how greatly soever the fluids might be diminished, the seats of disease will still obtain a preternatural quantity of fluids, and this disproportionate local circulation will be maintained as long as the disease itself continues. It is not, therefore, the making of a derivation of fluids to other seats that will cure a disease; but such derivation of fluids must, for this end, have a curative relation with the state of disease: it is always in our power to make such a derivation, but we do not find that diseases are always cured by it. The dependence of cure, by remedies of this class, is on their curative relation with the state of disease; and this relation of cure is by one of the following modes: 1st, If general excess of blood is a cause of disease in an organ which is predisposed to suffer (and

therefore displays local affection) from this cause, the diminution of blood may cure, by the removal of a cause. 2d, If the state of disease has been assumed, independently of excess of fluids, and by its relation with fluids compels a local excess of them, the diminution of fluids as a pabulum, or supporter of disease, may diminish its intensity. 3d, If the constitution of disease is dependent upon such supporter as a re-agent, the reduction of the quantity of fluids may also cure disease by removing a cause of its continuance. 4th, If the state of disease is not dependent upon the local quantity of fluids, as one cause which excites a local predisposition, or as a re-agent, the diminution of this quantity may act upon the more general principle of producing a change in the relations of the constituents of disease, and the results of a change on this general principle are hazarded on this as on other occasions on which it is acted upon; namely, the effect of a change in the condition of disease may be a resumption of the natural or healthy assimilation of life, or the state of disease may be modified; or successive disease, or a permanent substituted assimilating state of disease, may supervene; or a progressive change, which may terminate in death.

But we have remarked only one mode of cure by remedies, whose primary operation is on a related seat; namely, that by derivation of fluids. This mode, comprising the sensible effects of such remedies, exemplifies only the indirect relation of remedies with the state of disease which belongs to the department of life. The properties, or state

of life in the seats of the operation of remedies, suffer an affection, inferred with great certainty, as considerable, and perhaps more important, than the increased derivation of fluids to such seats, which it produces. If this affection, or change in the department of life, is related with the state of disease belonging to the same department (a sympathy which has before been shown to be more than probable), the mode of cure, by derivation of fluids, is only one, and perhaps a subordinate relation of remedies employed upon one seat, with disease in another. The influence on disease, by change in the properties of life belonging to a related seat, is liable to all those modes which have been said to belong to the examples of related disease. In this therapeutical view, our business is with the results of the change in the properties of life in a seat of the action of remedies, in relation with their state in a seat of disease: in this case the relation of remedies is direct, so far as the department of properties with which they are engaged is the same; and indirect, only by producing their effects through a secondary seat. The results of preternatural affection are, 1st, That no relation may subsist between it and disease in another seat; in which case, the remedies produce their own effect on the seat of their employment, and the accompanying disease continues its course. 2d, If the states of the seats of preternatural affection and of disease are related, the effects might be to derive properties of disease to the seat of preternatural affection, as fluids are also derived by the influence of such affection; this may be

termed an artificial metastasis, and is inferred from analogy to the examples of the spontaneous translation of disease. 3d, The state of disease may be changed by the influence of properties from a related seat under preternatural affection; and the results of this change may be, first, an affection or modification of the disease, without change of assimilation, the consequences of which affection are manifested only so long as the remedy continues to be supplied; second, the state of health in the seat of disease will be resumed; or, third, a substituted form of assimilating disease may be produced; or, fourth, successive states of disease may succeed, terminating either in recovery or death.

5. Those remedies which remove known causes by direct or indirect relation with them. The known causes of disease, except those which are external, are the effects of disease, which, by their re-action, maintain a state of disease. Among the most obvious of these causes are those which are the objects of surgical treatment, as urinary calculi, stricture, strangulated hernia, &c.; and in the department of medicine, gall stones, scybala, and other accumulations in the intestines, perhaps schirrous states of organs, or the changes of structure, sometimes produced by inflammation.

The removal of the first is a mechanical operation; or, in some instances, like that of the latter, is, by the intervention of a previous agency on the properties of life, as in the cure of strictures, &c., upon the condition of which, secondary disease might depend. We may, in some instances, easily

remove the material effects of previous disease, by which a disordered state is still maintained; this state is merely a disorder of affection, when it acknowledges such dependence; but if the change of structure is dependent upon a condition of life, the cure of the phenomena dependent upon affection can be accomplished only by a change in the assimilating state in the department of life by which such organic change was produced and is maintained. The accomplishment of this design will be by one of the modes already declared.

6. Those remedies which cure by mixed processes resulting from their relations with a state of disease. The remedies, whose operation is on the seat of disease, or on a related one, as well as those exemplifying the double relation with life, and with the organization, are comprehended in this class. It is probable that most remedies operate by mixed relations; that they affect the state of disease, both by direct influence on the department of life, and that they also affect the state of life, by an operation upon the hydraulic or mechanical states with which life is related. Thus, it is probable that mercury, steel, the mineral acids, bark, arsenic, &c., produce their effects on disease, both by an operation on related seats, as the liver, stomach, &c., and on those of disease, through the medium of the circulation; and that bleeding, purgatives, emetics, mercury, carried to the extent of affecting the glandular system, nitre, digitalis, &c., exemplify the mixed agency on disease, arising out of the relation both with the con-

stituents of the vital and hydraulic, or mechanical departments.

The exhibition of medicines, then, appears to have the following dependences and results.

1. There may be no relation between the medicine and the state of disease.

The consequences of a relation between the remedy, or rather preternatural influence, and the disease, may be,

1st, A mere affection of the disease by the influence of the remedy, in which case the disease may be modified, or even suspended, only so long as the operation of the remedy lasts, or so long as it is repeated, and then resumes its former state.

2d, A change in the *assimilating* state of disease. The modes of this change are, 1st, by a relation of the remedy with life; 2d, by a relation with the constituents of the hydraulic or mechanical department; 3d, by an action on the seat of the disease; 4th, by an operation on a related seat; 5th, by all, or some of these modes, mixed.

The results of change of the assimilating state of life, are, 1st, recovery of health, or resumption of the natural assimilating state; 2d, substitution of another permanent state of disease; 3d, a succession of states of disease, by progressive causation, terminating either in recovery or death.

The speculative principles of therapeutics here exhibited admit of many subdivisions; and the processes of cure, and the modes of the operation of remedies, may be much more minutely traced. Such additional detail would, perhaps, afford no

compensation for the perplexity which must at least appear to belong to a subject so curiously analysed. It is my present wish to avoid such seeming perplexity; and it was a collateral object in entering, either here or elsewhere, into speculative disquisitions, to recommend modesty to certain false theorists, and to teach the sweeping systematics of the day, who will have nature to be as simple as their own conceptions, that their habits of easy assumption will be productive of nothing but error; and that if they would trace the processes of nature with accuracy, and represent them truly, it must be by a comprehensive examination of facts; and by a consideration of their testimonies, which receives no false bias from a pertinacious adherence to some favourite preconception.

CHAPTER X.

PRACTICAL PRINCIPLES OF THERAPEUTICS.

The condition of health has been defined, loosely, to be a certain state of living bodies which is most agreeable with their objects, and consists of a due and easy discharge of the functions of their several organs. This state is liable to many deviations; and these deviations are expressed by the terms predisposition and disease. These two appear to be only different stages of change; the former is inferred, agreeably with *à priori* conclusions; the latter is declared by symptoms. The deviations from the state of health, which constitutes disease, are denoted by a great variety of symptoms: hence, there is equal variety in the states of disease, of which these symptoms are the signs, and by these signs diseases are respectively characterized.

It is the design of therapeutics to restore that state of health which has suffered the deviation characteristic of disease. But as the precise nature of the change from health to disease can rarely be known, even imperfectly, because the agents are, for the most part, not objects of the senses, so the plan of therapeutics is not one of direct causation, which consists in removing or supplying ascertained causes, but proceeds upon

the mode of indirect causation, or accomplishes its object generally through the medium of relations, which we have seen to be of a complicated kind.

Our information on this branch of science, as on all others, must be founded upon experience: and experience in medicine is this; that certain states of disease are declared by certain signs; and that certain remedies have restored health, when employed on states of constitution, denoted by these signs. But, as the results of the employment of such remedies in connexion with the same signs are irregular, we are led to conclude that the analogy between signs, or symptoms, in different cases, does not prove an analogy in the state of disease; from whence we conclude further, that the entire state of disease is not declared by symptoms; and that with resemblance of symptoms, there may be essential difference of states. This is one ground of deficiency in the testimony of experience.

The practice of medicine, comprising rules of treatment, is a theoretical superstructure, raised upon the basis of experience. And it is liable to the general defects of inferential information, which is perhaps the largest portion that we possess on any subject. Inference always presumes analogy. The analogy which is presumed is of those particulars which are unseen; and this analogy, in such particulars, is presumed from our former experience of their connexion with those which are seen, or otherwise witnessed. Thus, in medicine, certain symptoms are declaratory of a state

of disease; certain remedies have been found to cure the disease thus declared: when, therefore, we meet with the same symptoms, we infer the same state of disease, and we employ the same remedies, upon the conclusion, that the same symptoms denote the same disease; and that the same remedies will cure the same disease.

But the perceptible symptoms might be the same, and other dispositions in the state of disease, not manifested by symptoms, may be different. Hence our remedies would fail, if administered on this ground of analogy, for the reason that the analogy is perfect only in the visible signs, but is imperfect in other respects. It is not, therefore, as our inference would assume, an employment of the same remedy to cure the same disease; but an employment of the remedy upon some points of analogy, on the presumption that the analogy obtains equally in all other respects. This has been just said to be one ground of the defective testimony of that which is called experience in medicine.

But this is not the only ground on which this testimony is defective. Not only are states of disease sometimes different, when the sensible signs are the same; but the sensible signs themselves have frequently, perhaps generally, only a partial resemblance; that is, a disease which we meet with to-day, may have so great a similitude to others which have occurred to us before, as to appear to be of the same character: but with many points of analogy there may be also some of difference; and hence we are precluded the inference

that the remedy which has succeeded with states declared by certain signs, will succeed in an example in which these signs are analogous in some, and different in other particulars.

Hence the experience in the signs of disease, and experience merely in the final results of remedies, appear to afford but an imperfect instruction; for if certain remedies are given only in connexion with certain symptoms, the same remedies would not be given, upon the presumption of analogy, if the symptoms or signs of disease were in many respects different; and if the case were one exhibiting rather diversity than similitude to our former experience, we should have no remedy connected with such signs; and should therefore be under the necessity of withholding means altogether, or else we should proceed in their employment without a guide.

It appears, then, that the defect in this testimony of mere experience arises from the imperfection of the analogy between states of disease: and as we are often called upon to treat diseases in which, if all the circumstances were enumerated, there would, perhaps, be as many points of diversity as of analogy to our former experience, so it is necessary to obtain a collateral assistance of some kind.

In addition, therefore, to the particular indications which we derive from the resemblance of certain diseases to each other, we are led to examine *the dependences of cure, or the modes of the operation of remedies.* The result of this examination is also founded upon our experience; but instead of directing a practice on the presumption

of a precise resemblance of certain states, as declared by similar signs, it consists of a deduction from our *general experience*, both in the signs of disease, and in the operation of remedies; and is founded upon a *general analogy of dependence*, instead of a particular one in the signs of individual diseases.

The deductions from our general experience are, like the indications derived from resemblance in the signs of particular diseases, a presumption of analogy; and the result of remedies, employed upon this presumption, is according to the truth or fallacy with which the analogy is inferred. Thus we observe it is the common operation of remedies, under a great variety of circumstances of disease, to produce certain effects, on which the cure of disease in general depends; hence, overlooking as unimportant, minor particulars of resemblance, or diversity, between diseases, we are instructed by a general experience, how to produce certain effects; and the same general experience teaches us that the termination of disease, although greatly diversified in its forms, has a general dependence upon such effects. Hence we obtain a general curative indication: and the employment of remedies, conformably with such general indication, is said to be a treatment upon *general principles;* as that is a treatment upon a *particular indication*, which is founded upon the resemblance of signs, which may be supposed to be declaratory of a similar state of disease.

The particular indication of cure is suggested by the resemblance of diseases to each other,

which have ceased under the treatment by certain remedies. The analogy in certain symptoms affords a presumption of a similar analogy of dependence; and hence the same diseases, or those bearing a conspicuous resemblance in some particulars, are treated by the same remedies, the resemblance of signs being presumed to prove a resemblance of state, and therefore a common dependence. The discrimination of analogy between diseases is the province of observation; and the field is an ample one: the discrimination of analogies and differences between respective diseases is termed, in medical language, *diagnosis;* and the points of analogy, or difference, are called diagnostic signs.

Treatments are sometimes instituted upon a supposed analogy between the *causes* of disease; and sometimes without reference to causes, but merely upon an experience of results. The presumed causes of disease are those secondary effects of disease which are displayed by the state of the structures, by that of the circulation, or by preternatural products: the treatment, without reference to causes, is founded upon the resemblance of the signs of disease, and the experience of success, by the operation of remedies, employed in connexion with such signs. In the latter cases, causes are not sought for, and the general principles of cure, deduced from a combined experience of the nature of disease, and of the operation of remedies, are neglected; and hence this practice has been called empirical.

It has been the general object of medical in-

vestigation to discover analogy, in some essential respect, between diseases, in order that the treatment which has been found successful in one instance might be extended to another, upon the presumption of this analogy between them, and in the expectation of a similar result.

Particular indications of cure are illustrated generally by the cases of surgery, the objects of which are, to remedy recognised diseases of the structures by certain treatments, which have succeeded in such cases; or to obviate the effects of secondary disease, by the removal of known material causes. In medicine, also, the particular indications of cure are very numerous. Thus, blood-letting is employed in inflammations, occupying different structures, from the analogy of this state, in its several examples, and the experience, that blood-letting is remedial of this state, whether by removing a presumed cause, or by a mode of operation which is not understood. Thus the analogy between fevers suggests a treatment of them by similar means. If more general disorder were connected with depraved abdominal secretions, aperients or purgatives would be indicated by the results of their employment in cases similar in this respect; whether these depraved secretions were supposed to be causes of disorder, or if their relation with connected disease were not inferred, this point of analogy would afford a particular indication.

This analogy of signs, for the purposes of indication, are minutely sought after: thus, supposing diseases to agree in the general circumstance of

depraved abdominal secretions, these secretions may be of different kinds, and the particular kind would be ascertained; and according to its kind, a different indication of cure would be suggested; and mercurial or other aperients would be selected, in agreement with the particular indication of these means. Thus also, in cases agreeing in the analogical circumstance of an accelerated circulation, the therapeutical object would be to diminish the rate of the circulation, and the means employed for this purpose are suggested by this particular indication. Thus also, some diseases would be treated, from their resemblance to others of the same character, by cicuta, by opium; and others of the skin, bearing analogy to former experience, by sulphur; and others resembling those which have been cured by arsenic, or corrosive sublimate, by these medicines, &c.

Thus the particular indication of treatment is founded upon the resemblance of a present disease with another of the same character, for the cure of which a certain treatment has been before instituted, with partial advantage, or with perfect success.

The general principle of cure is that which is deduced from the general or common nature of disease; and the common operation of remedies.

We are instructed of the general nature of disease, that it is a deviation from the state of health; that we do not know in what this deviation consists; that its relations are those which were declared in our examination of speculative pathology; that it is maintained by assimilation; that

remedies do not act, in general, on a direct principle of causation, but by a relation with disease, by which they subvert its state, and dispose the agents engaged in it for certain changes, among which is the resumption of health.

The only particular then in which all curative remedies agree in their operation, is, that they subvert the present condition of disease: and this furnishes a general indication of cure.

As the remedies which are curative of the same diseases differ, as well in their ascertained properties as in the phenomena they produce, in their employment for the cure of disease; and as they all produce the same final effect, so it must be concluded, that they accomplish the same end by different agencies. If their agency respectively is exerted upon the state of disease, this state must be modified by them in a ratio to the diversity of their agencies; but if the recovery of health is a result of all these changes, or of the modifications of disease, respectively, produced by each remedy, the state produced by each, although different, must have a common relation with the end. This common relation of different states with the same end is no less conspicuously exemplified by the different effects produced by remedies, all terminating in recovery, than by the different forms of disease; which, notwithstanding their diversity of form, tend to a common result, namely, that of the recovery of health; which exertion of a tendency among agents in connexion with different states, is said to exemplify a vis medicatrix. As all these different states, both the spontaneous

ones of disease, and those produced by different remedies, have a common result; so this result must have a common dependence upon these states, or must acknowledge the same law. The continuance of disease is dependent upon an assimilating state of life: if by changes in this state such assimilating state of life ceases, and gives place to a change which cannot maintain itself by assimilation, but is one of affection, and, as in the case of the exhibition of medicines, dependent upon a supply of the causes of such affection, the natural assimilating state may be resumed; the former one of disease having ceased, and the preternatural one, produced by remedies, being dependent upon the continued supply of external agents.

This common effect of remedies, or of related processes of spontaneous disease, exemplifies the subversion of disease; and furnishes a common design of treatment, in the absence of remedies which are indicated, conformably with our experience, with a view to more precise effects, or with a view to the fulfilment of ascertained relations by curative means. The test of the subversion of a state of disease is the cessation of all, or the most characteristic, of the symptoms by which it was identified: the final result will then depend upon the disposition of the agents, or causes, concerned in the disease.

But the cure of a disease requires not only a subversion of the state which produces symptoms, but a subversion of the *disposition* which led to

the occurrence of those symptoms; and this latter is by far the most difficult object to accomplish in many cases. Thus the existing symptoms of syphilis may cease under the exhibition of a certain quantity of mercury; but the remedy being discontinued, the disposition to syphilitic action not being also subverted, analogous venereal disease might be produced by it. Thus, also, typhous fever may cure for a time an habitual dyspepsia; or rather, its symptoms may cease; but the disposition remaining, the same symptoms may afterwards be produced from it. Thus, also, bleeding, purgatives, emetics, mercury, &c. may remove all the symptoms of dropsy; but unless the disposition to disease is also subverted, the symptoms of dropsy will be reproduced. There are no instances which more frequently suggest the axiom, "*naturam expellas furcâ, tamen usque recurret,*" than those of disease. In general, as illustrated by syphilis, dropsy, in a few diseases of the skin, &c. the remedies which cure the symptoms, if carried to a greater extent, will subvert also permanently the disposition by which they were originally produced, and may be produced again; and the curative treatment, with respect to chronic disease, will, in general, in all these cases in which the predisposition may outlive the symptoms, be defective, if it is not conducted with this view, or carried to the extent of accomplishing this design.

Upon general principles we employ a remedy as a related agent with respect to the condition of disease. Unless a medicine is a related agent, it

has no influence on disease: it may produce its own effects, and the disease may continue an uninterrupted course. That a remedy is a related agent, is proved by its affecting the disease, either increasing or mitigating its severity, or modifying its symptoms, producing new ones of disease, or changes indicative of healthy actions in the seat of the disease, or in connected ones. In this quality of a related agent, a remedy is liable to have with respect to disease one of the following results: 1st, a mere affection of the state of disease, which continues only until the influence of the remedy is past, or as long as the remedy is repeated; 2d, a change in the assimilating disposition of the disease, by which the state maintains its change, after the operation of the remedy is passed, or when it is no longer repeated. The results of a change in the state of disease, which maintained itself by assimilation, are, 1st, the recovery of health; 2d, a permanent substituted disease, local or constitutional, in the same, or in a different seat; 3d, successive states of disease, terminating in recovery or death.

It has been seen that the spontaneous dependence of disease is upon the assimilation of this state, which the principle of life has assumed; and upon the re-action of the mechanical or hydraulic changes which this state of the principle has produced. Certain phenomena of disease may depend upon this re-action of the alliances of life; and these phenomena may be those of affection, or of assimilation; and as the causes of these pheno-

mena are of the material kind, they are sometimes recognised, and are remedied by a treatment which respects the removal of an ascertained cause. But the change in the mechanical or hydraulic department is itself maintained by an assimilating state of disease in the department of life: hence the treatment which cures the phenomena dependent upon a material cause of disease may fail in subverting the primary assimilating state of disease, on which the mechanical or hydraulic changes and re-agents depend.

The removal therefore of a re-agent, or cause of disease, in the mechanical or hydraulic departments, will not cure the entire state of disease, unless the remedies are also capable of subverting the state of the principle by which the secondary disease, or change in the mechanical or hydraulic departments, was first produced, and may be renewed, when the remedy is discontinued: but those phenomena only will cease upon the removal of a mechanical cause, which depend upon its reagency; and not even the whole of these phenomena, if the relation of the re-agent with life has been to dispose it to other disordered assimilation. Thus purgative, emetic, and diuretic remedies may cure the mechanical state of ascites, and all the phenomena dependent upon this state, as difficulty of respiration, &c. may cease; but if the state of the principle which produced the disease is not also subverted, it will be reproduced, and maintain the same mechanical and hydraulic effects. On the other hand, if this state of the prin-

ciple has ceased, a remedy, the object of which is to remove ascertained causes, may be entirely curative of the state of disease.

It may be said, therefore, with the exceptions before expressed, that the common effect of remedies is to subvert a state of disease, not by supplying, or removing ascertained causes, but by a related agency, which changes the assimilating state of disease; and this related agency may have, with respect to the disease, one of the above mentioned results. The dependence of either of these results is upon the relation between the remedy and the disease.

Many, and even opposite, remedies, agree in subverting or changing the present condition of disease; and this common effect may be followed by a common result; or the causation which will ensue will be the same; because, although the agents appear to be different, the real properties which are engaged in the relation are the same; as the structures of a man, or other animal, are the same, however diversified his food; the properties or substances which constitute the variety being only associated ones.

But the real agents engaged in the relation by which disease is subverted may also, as we find from experience, be very different, and consequently they will dispose the processes of disease to a termination in other substituted disease, or in recovery, or in death. We are, therefore, no further instructed in the cure of diseases by a general principle, than that the assimilating state of disease must be subverted; and the terms of re-

covery are, that the artificial state or change in the disease, produced by the remedy, should be disposed to this termination: hence a curative relation with the state of disease.

Although, therefore, many remedies concur in the general effect of subverting disease, yet as the results of this subversion of disease may be either of those numerous ones before specified; so, in a treatment, which entertains a design with respect to only one of those results, namely, that of recovery, we cannot employ agents, indifferently, that have the common effect of subverting the state of disease; but we must select those which dispose to one particular result, namely, that of the recovery of health.

We perceive, then, that different treatments, although agreeing in a common effect to a certain extent, may dispose to different results, from whence it is to be concluded that remedies, although agreeing in one effect, which is common, have different relations with the processes or causation of disease; and therefore the object of therapeutical investigation is to select the remedies which have a curative relation with the state of disease, rather than those others, which, being equally capable of changing one diseased condition and substituting another, are followed by processes which end either in substituted disease or in death.

The *general principle*, or indication of treatment, is the subversion of a state of disease; but it is by the *particular indication*, or that founded on individual experience and analogy, that the

treatment is selected which will lead to the curative result. Thus, general indication may supply the place of particular indication, where our experience is altogether defective; and without a knowledge of a remedy, which, according to former experience in similar states, would dispose to recovery, we aspire only to the design of subverting the state of disease; and the consequences of such subversion may be recovery, substitution of disease, or death. These alternatives are rarely hazarded, or in propriety should never be hazarded, unless the fatal tendency of the disease, if left to a spontaneous course, is demonstrated by our former experience; and the last of these alternatives, under a treatment upon this principle, will rarely be hazarded, if such treatment is conducted with attention to the effects of the remedies, and with due regard to the tendencies which analagous effects have displayed. We cannot, upon the general indication of treatment, anticipate the result with much certainty; but we, nevertheless, frequently institute a treatment chiefly on this principle; and the treatment is instituted in a confidence of this sort: that, whereas the tendency of the natural disease was to death, the tendency of a change in this state may be to recovery; that if the state of disease consists only of that which it is made by the remedy, the recovery of health may succeed to the discontinuance of such remedy. The confidence, therefore, for the recovery of health, under the preternatural change of a state of disease by remedies, is reposed in that which has been called a

vis medicatrix, which is a tendency arising out of the relations of the agents engaged in maintaining life and health.

But this tendency, as we know from the occurrence of other results, is by no means uniform. It often happens that the other results ensue from preternatural affection, instead of that of recovery; and it still more frequently happens that the disease kills, or the mixed agency of the disease, and the remedies, kills, before the state of disease is subverted; and in such cases no opportunity is afforded for the exertion of the vis medicatrix. It is, in the phraseology of Newmarket, running neck and neck between the disease and the treatment, whether the treatment first subverts the disease, or whether the disease first kills the patient.

The object of therapeutics is the cure of diseases. This is attempted upon two indications; a general one to subvert an assimilating state of disease, whatever may be the seat of this assimilation (upon which all the phenomena, except the material effects of disease, ultimately depend), and a particular one, to select those remedies which, in similar states of disease, have been found, in our former experience, to be curative. The selection of remedies is therefore founded upon our past experience of results, in connexion with their employment; we choose those, which, in analogous cases, have, according to our experience, been curative; and reject such as have led to other permanent forms of disease, or have terminated in death. In the absence of every ground

of selection, we design the subversion of a present state of disease; and trust the result to a disposition to recovery, which frequently prevails, when the spontaneous or natural state of disease has ceased.

It rarely, however, happens, but that the general is assisted by the particular indication of treatment; for however diversified states of disease may be declared to be by their phenomena, some point of analogy to our former experience in general inclines us to attempt the change of the state of disease, rather by one method than by another; and if the particular assists the general indication, in the choice of means, the general indication helps to direct the views with which those means are employed, and the extent to which they are to be carried. It is probable that diseases are cured on the general principle of subverting the state of disease more frequently than we suspect; for, although certain remedies only appear to have curative relations with diseases, this may happen because such remedies only are capable of subverting the condition of disease, without exemplifying a peculiar agency in disposing to one or other of the results which have been said to be the alternatives of a change in the condition of disease; thus acting by a common remedial effect, rather than by a curative efficacy which is peculiar.

Thus, therapeutics consist in the employment of agents whose relations with disease are to dispose to the recovery of health: these relations are confirmed by experience, and the agents are directed

by the observation of analogy between states, with which their curative relation has been proved by this test. Hence the principles of therapeutics, agreeing with the indications of treatment, are general and particular; and both are merely abstract rules, which consist in the definition of experience, and the presumption of general analogy, from resemblance in particular signs.

THE END.

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